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JOURNAL OF THE HISTORY OF MEDICINE AND ALLIED SCIENCES

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(See inside front cover for detailed announcement)

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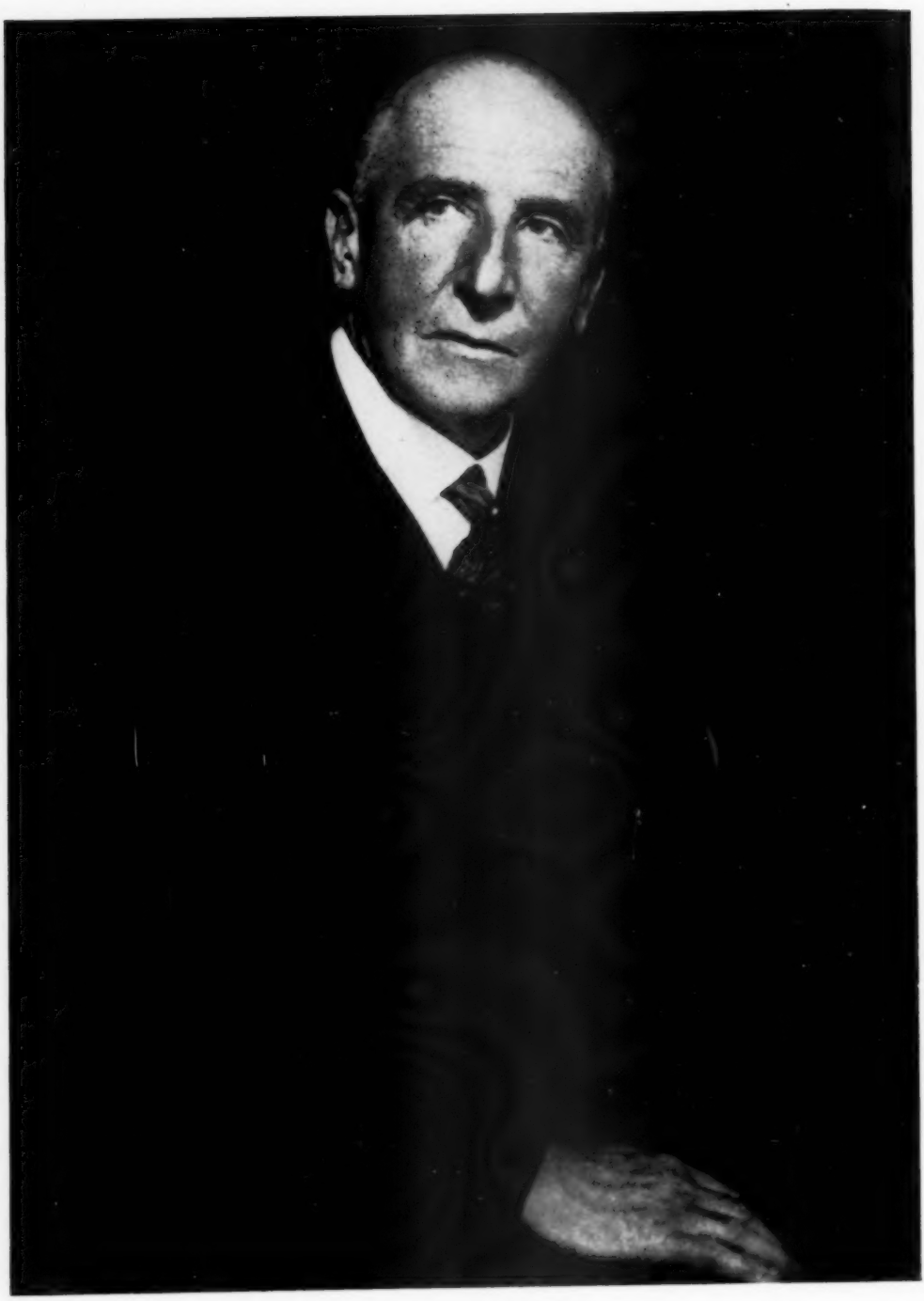

ROBERT MEYER

A Short Abstract of a Long Life

TO MY FRIENDS IN THE UNITED STATES OF AMERICA.

ROBERT MEYER*

I NEVER planned to write an autobiography,[1] although my young friends in Berlin asked me to do so more than once. I was of the opinion that no man should describe or tell his story who was not in public life and influenced it. It was also clear to me that I would be vain to write another autobiography just because events and persons which influenced my life appear important to me while to a stranger they are quite unimportant. For this reason I never took notes of any events in a diary and preserved no letters. Some of the letters would now be of interest because I had a life-long correspondence with many pathologists and gynecologists. Some of them I should have preserved because they were of general interest especially those from Richard Semon, the editor of *Zoologische Reisen*. He wrote on the *Inheritance of Acquired Characteristics,* a subject in which I was interested. And I wrote an abstract of that book. It happened that a brother of the author, Sir F. Semon, a famous laryngologist in London, read my article and thus learned that his brother was a famous

Editor's Note: When the doctor speaks of things he has done and seen and known, whether these be concerned with his own profession, or with some other human wisdom or folly, what he has to say always remains personal and imbued with the immediate breath of being, for the doctor, above others, is closest to life and death. Doctors, holding the keys to many doors, have often been in a position to communicate uniquely important information about events in which they played a part, or about people and happenings of which they were observers. Such writings form priceless historical sources, in many cases so vividly created that we seem to take part vicariously in the events described. For this reason we are publishing in this issue the first installment of Dr. Meyer's autobiography, which we believe to be an important source for the medical history of our time. In a later issue of the JOURNAL (Winter 1948), we shall present an interesting contrast to Dr. Meyer's story by publishing the autobiography of Johann Peter Frank, in a translation prepared by the Editor.

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[1] At the request of some friends of mine, especially that of my good friend Emil Novak, I have written a short account of my life. He used this material to give in his well known masterly style a panegyric before the American Gynecological Society in 1946. He added also a bibliography of most of my publications. I wish to thank him sincerely for his splendid endeavor. The following autobiography is a more complete record of my personal life and gives more details of my work than was possible in Novak's address. My personal life loses significance with advancing age while my work eclipses it. I have written this short treatise in my own would-be English and Dr. Stuurmans, a former student and now a good friend of mine, has revised it.

man. Before this, Sir F. Semon had thought that his brother had written sheer nonsense. Among these writings were the famous books, *Mneme* and *Die Mnemischen Empfindungen*. The first book was a brilliant addition to the knowledge of human psychology; Ernest Haeckel called it the best book of the year (1912). Richard Semon wrote me what his brother thought about my abstracts and sent me his other books too. Later he became very much interested in my articles about deception and self-deception (*Berliner klin. Wochenschr.* (1914), Nos. 23 and 32). Thus, I had a most interesting correspondence with him, a man who had an extraordinary and clear mind until the end of the war (1918) and the end of his life. When Germany was defeated in 1918 he lay down on a black white and red flag and committed suicide. He was an ultra nationalist.

I have had an exceedingly beautiful life which would be worth living once more. I have had no sorrows except those I made myself and they were unimportant. I have always had the feeling in difficult situations that things would come out well, and they came out right. I let life approach me, made my decisions, and they were right. I did not seek or apply for any position. I had nobody to ask for recommendations. I was in every respect independent. I worked for my satisfaction. I was an assistant only for one short period and never had the ambition to be a "privatdocent" of pathology or anatomy as was expected of me. I was an autodidact. I had no plan, no connections, when I began to study pathology at an age when most people are already established. In retrospect I find that this seemed to me quite reasonable at the time I did it. But why I did it I do not know; it seemed to me important. It happened that life offered me something, and whether I took it or not, it was right.

Now I shall try to sketch my life in proper sequence. I remember my grandfather (born in 1780), a former banker, when he was eighty years old. He had already retired from business and was attempting to make artificial diamonds without scientific basis. He was born in Speyer, in the Bavarian Palatinate, but lived in Hanover. On the first floor of his home was his bank. On the second floor lived my grandmother, an invalid for eighteen years. During my lifetime I have never seen a woman crippled to such a degree as she was by arthritis deformans. In spite of that she had an especial charm and dominated a family of eight children and had wonderful cookies baked for twenty-eight grandchildren. My father was born in 1822, the oldest of the family and, because there were six sisters

he was not allowed to study as he wished. My grandfather was too old to manage the bank. Later on, two of his daughters married bankers. My father and his brother could then buy a rubber manufacturing plant. Neither of them inherited the longevity of their parents as did the six daughters.

Of my mother's parents and their six children, none reached more than their seventieth year. This family was extraordinarily handsome and elegant. They had been leather dressers for generations, but I do not know much about them except for one uncle, since they lived in Frankfort-on-the-Main.

My parents were married in 1860. They already had a son and a daughter when I was born on January 11, 1864. A second brother and another sister arrived later on.

Since it was fashionable at that time to have a wet nurse in a wonderful looking costume, I yielded to having one whom I loved. She loved me too and came to see me on my birthdays for many years. Her costume was that of the peasants in Hessen-Cassel where she wore it only occasionally on Sundays. But while in Hanover she wore it all the time to show off and to become acquainted with other maids. She had an illegitimate child which was in no way a disgrace in her home. To her people it was a legitimate child. She had to prove her fertility before she could get married because to have descendants was to a peasant an indispensable blessing. Besides, she had to earn her trousseau by nursing.

When I was five years old my father took me once to Frankfort-on-the-Main and I remember having seen the whole family including the parents of my grandmother, Mr. and Mrs. Wolff. Later on I was told that I greatly resembled my great-grandfather. All the company was in full dress but I do not know why. This is my earliest memory. When I told my mother about it as a grown-up she did not believe me until I also told her that the furniture of the drawingroom was covered with blue silk. I have an even earlier recollection of my fourth birthday but it was falsified by my telling about it the next year and the year after, thus it no longer represents an original impression. There are a great many such records in autobiographies but they cannot be considered original.

I remember quite clearly from my seventh year (1870-1871) that we were permitted to take cigars to some French prisoners, white and black. They were from Algeria and were called "Turkos." And they were busy making "charpie." That is an old French term meaning making lint by

hand from fine linen; it was used for bandages. At that time we were living in a house with a beer garden in the rear and many lamps of various colors which impressed us beyond measure. German patriots then could indulge in patriotism by drinking beer (also beyond measure), by singing all night, by fighting with beer glasses. They also used to throw each other into the basin of the fountain of the garden. To us that meant patriotism. At ten o'clock the night watchman appeared with a lance, a lantern and a horn, accompanied by a dog, usually a Pomeranian. He began to blow and sing: "Listen you people and let me tell. It now is ten as rang the bell. Put out the fire and the light lest fire break out in the midst of night." Then he blew his horn three times. It was exactly as you hear and see it in a performance of the opera *Die Meistersinger von Nürnberg*. In order to know Germany during the eighteenth and nineteenth centuries, the wonderful pictures of Karl Spitzweg should be studied. The night watchman was still on duty when I was a boy of twelve years.

In 1864 Hanover still had a king, a Duke of Cumberland, whom the wicked Prussians drove away, making a Prussian province of the land of Hanover in 1866. They did the same to other territories.

In my early years I was a very delicate boy, hence I received private instruction until my eighth year of age. When I did go to school I had to learn the difference between the adherents of the former King and those of the Empire. The firm supporters of the King, the so-called Guelphs, founded a political party which for many years played the role of opposition in the Reichstag. It found its echo in the school. I was not one of the Guelphs, since my parents favored the formation of the Empire in 1871. The triumphant processions after the war against France (1870-1871), and the annual celebration of Sedan on the second of September, left a deep impression on the susceptible mind of a boy. Many teachers had been officers in the war and were apt to become very enthusiastic about the struggle which ended so happily that we could live in peace for forty-three years. We were genuine idealists, enthusiastic about the heroic deeds of antiquity as well as those in German history.

On the other hand, we were impressed by the old castle in which a Prussian prince resided and by the royal stable of proud yellow-white horses, ideal animals, the famous Isabelles, and by the royal pack of spotted dogs which served as fox hounds. These same dogs came on the stage in

Tannhäuser when Albert Nieman, the phenomenal tenor, and five other singers rode in on horseback in an unforgettable scene.

The peace treaty of 1871 returned Alsace and Lorraine to Germany after the provinces had been in French possession for almost two hundred years. The people were not asked whether or not they agreed. They still spoke German in Alsace but it was an awful dialect mixed with many French words. After Mr. Bleichroeder, a famous banker, had been consulted as to how much France would be able to pay during the following three years, while the German occupation forces would stay in the country, the sum of five billion marks was agreed upon. This was a sum of money unheard of at that time, but it was paid up by September 1873, as far as I remember.

Politics was not a subject of conversation in our home. "Politics spoils the character" my father used to say. Besides, he was a very busy man. He and some friends founded the Caoutchouc Continental Manufacturing plant, internationally known as "Continental Cords" later on. He was a good friend of his workmen. They presented him with a silver goblet which I preserved until I had to deliver it to Hitler. I do not know what use he, Hitler, made of it. On Sundays in fair weather we five children made excursions with our parents in a mighty carriage. My father loved to drive and ride on horseback. After the war we had a wonderful house in Gothic style built by Professor Oppler. It was mentioned as one of the sights of the town. We loved the dining room especially because of a stove with a chimney going through the armor of a knight, and a gallery of paintings on a broad staircase which ran through the whole house. The pictures represented scenes from Shakespeare's dramas. I was especially impressed by those pictures because I could compare them with stage performances. Although at that time Hanover had not more than one hundred thousand inhabitants, we had a good opera and theatre, and all Shakespeare's plays were given in a series of performances.

In my childhood I was not often ill but I do remember having had an infection in the end phalanx of a finger, which our doctor cured by a poultice of bread cooked in milk. A furuncle in my ear he treated with hot linseed. The day of my entrance into school was of special importance in my life. My neighbor on the bench was Conrad von Holleuffer with whom I walked home that day and all the following days until we left school at eighteen or nineteen years of age. We shared everything. We had much pleasure and much school work at home, and played many

foolish tricks. He had three sisters and a brother and soon the two families were all mixed. Later on he loved my older sister, but for me his sisters were comrades. I loved Mathilde Krieger, then fourteen years old, with whom I learned dancing. A lovelier girl did not exist than that ideal, but I was too shy to confess my love for her. Whether she guessed it I have never known, but should I see her once again I would bless the great grandmother for so much happiness. Conrad became an officer like his father and his older brother and sometimes had to change his garrison, but we remained friends until we were 75 years of age, when I left my country. He lived in Berlin a long time, so our families became intimate and one of his daughters learned microscopical drawing from me, although I myself was not able to do it.

Much depends on your teachers whether school life is happy, indifferent, or unhappy. Our teachers were, in the main, our friends and were regarded as gentlemen. They were a select group among whom were some prominent authors. Professor Wiedasch had written in hexameters a well known translation of Homer. Another, Ehrlenholz, was an excellent mathematician and physicist; in both of these fields I was markedly talented. I excelled also in gymnastics and singing but otherwise I was a mediocre student. We also had, in the lower classes, a teacher who taught us religion and natural sciences such as botany, zoology, and geography. His whims made the whole town laugh. For instance, in religion he used to make clear the difference between God and man by saying: "God has not only the name of God but he is God. That is the decisive difference between Him and human beings of whom one man is called Smith, but he is not a smith, he is a doctor; another is called Baker but is a hairdresser; a man called King is a beggar; and one named Satan is a pious clergyman; but God is not only called God, he is God." This, presented with hyperpathetic voice and gestures, provoked the sneers of boys nine and ten years of age. Many stories of him I keep still in mind, but they are not parlor tales. He also had a cane which he used expertly. I can recommend his method for home and school. His predilection for geography was primitive but true and when death finally came it found him in bed tenderly caressing his beloved globe.

We were fortunate to have a teacher like Professor Friedrich Grahn. We had him only privately, but in the school where he taught he impressed every boy by his shining face and his radiant eyes. It was quite the same whether he spoke of Roman or German literature, of gymnas-

tics, of the theatre, or of politics; he always did it with such enthusiasm and good sense of humor that he captivated the whole audience. He was the mightiest influence of my life during school years. He had the birthdays of his favored boys listed on a calendar and he congratulated each of them for many years after school days were over until he died in his ninetieth year. He also made trips to visit them but finally he remained in Berlin where he matriculated at the University as a student in his eighty-fifth year. He was often our guest then, as lively as ever.

I cannot dismiss my youth without mentioning a man who, together with his wife, were friends of my grandmother in Frankfort; although fifty years older than I, he influenced me by his boundless optimism. He was Otto von Corvin who fought with his pen and sword. As a revolutionary he had to go to America in 1848 where later he participated as a Colonel in the Civil War. He then came back to Germany and visited us often. He resided and wrote in Leipzig. One of his books, *Pfaffenspiegel* (*Priest-Mirror*), caused a sensation by its frankness and sharpness. When I studied in Leipzig later, I was a welcomed guest in his home and found him in the midst of some well-known German authors. Because of his lively temperament and brilliant conversation he was the center of society although he was more than seventy years old.

In my tenth year I suffered a serious knee infection, having been struck beneath the patella. A neighbor boy had used a lath pierced by a nail as a weapon. After a sleepless night, one of my father's good friends, Professor Stromeyer, came. A man with an impressive head, he had introduced operative orthopedics; a monument was erected for him in Hanover. My father had a bust of him which I preserved and later gave it to the Surgical Clinic when I had to leave Berlin in 1939. I had to lie in a tremendous bandage for many weeks. To keep myself amused I learned to play the violin. I was no prodigy but persevered. That was indeed a trial for the whole household. I had a good teacher, Professor Petri, from Amsterdam and later learned to play Beethoven's sonatas. One day, after having played one sonata, my teacher said, "This time you played it very well." I was surprised because I did not realize that I had played it at all. I had been completely absent-minded. I was very often absent in spirit and had great difficulty in concentrating and listening to the teacher. I learned French and Italian easily from my sisters' governesses.

My sisters also went to school, first to a private institution, later on to public school. They were never permitted, even when they were grown up,

to go to or from school without a chaperon. When they visited friends they were also attended by a governess or by a coachman or, after they were older, by one of us boys. The coachman had to keep a distance of five steps behind my sisters. Skating, too, was permitted only with a chaperon. This was taken for granted until they were married, one at nineteen, the other at seventeen years. My older sister was divorced after a child had been born, and then married the same man again in the same year of her divorce. I always thought that a rare event, until I happened to read that the statistics recorded such occurrences nine times for one year in Berlin alone. This sister was quite a character, very elegant but rather superficial, and the only one who was not musical. Once, when she stayed in a boarding house, she had forgotten to change her left stocking for the evening. A gentleman was the first to notice it. She was not a bit embarrassed but said, "You do not know it but it is the latest fashion to have two different stockings." The next evening a number of lady guests appeared with hosiery according to the latest fashion. I will tell another story which is characteristic of my sister. She had ordered a beefsteak in the dining car of a train. The beefsteak was much too small but her complaints were of no avail. On the contrary, she was insulted by the waiter and the cook. She was alone in the dining car. She put down her complaint in great detail on some paper and put it together with the beefsteak into the small box for complaints against the dining-car service. After a few days she received a polite letter from the management and her money was refunded. She told this story to a friend who published it in a newspaper.

We were all musicians except my older sister. My younger brother Oscar was a brilliant piano player, my sister and her two sons professional musicians, my son a pianist, and my daughter an excellent opera singer. In this company I myself was the least gifted but I am very thankful to my parents that they encouraged me to study piano and violin. One day there appeared in our home an artist, a professor of painting and drawing at the "Technische Hochschule" (The Institute for Technology and Applied Art), to whom I had to show my childlike sketches and paintings. He did not say much but shook his head and finally uttered "No." My mother certainly intended to make a genius of every one of us. But I was ashamed and tore my sketches and drawings to pieces. Yet they certainly would have won a prize thirty or forty years later, when art had degenerated. I was just born too early.

Music played a large role in my home town and in our home. Both theatre and opera, subsidized by the crown of Prussia after 1866, were excellent. Hans von Bülow was for some years the conductor of the opera and the concerts. My younger brother and I seated in the fourth, i.e., the uppermost, circle, where admission cost about twelve cents, were his most enthusiastic audience. We had to wait until the door was opened and then storm the four flights to outrun the other people, because the seats were not reserved. Hans von Bülow was also a great pianist, who played all the sonatas of Beethoven in the course of five evenings. It was an extraordinary pleasure to hear him play. Nobody else at that time dared to play the last three sonatas because they were difficult to understand. He edited the sonatas with keen remarks. He is also known to have played the ninth symphony of Beethoven twice in one evening to make the public understand it. He could be frightfully sarcastic. We also heard Liszt, Brahms, Joachim, Saint-Saens, Adelina Patti, and Clara Schumann, to recall only a few names among the celebrities whose concerts we attended for our twelve cents. My younger brother used to play on the piano until late at night what he had heard.

A great event for us was the first performance of the *Nibelungen Ring* by Richard Wagner, with which Angelo Neuman dared to tour Europe. He had engaged the best singers and a wonderful orchestra. The audience in Hanover did not know what to do with that music but applauded the singers heartily, and the critics amused themselves with vile witticisms. For us the impression was overpowering, although, to be sure, we could not understand all that Wagner meant. But I remember that we could not sleep afterwards.

Sometimes we heard the blind Professor Thürschmann, a fine looking man, reciting a cycle of the dramas by Aeschylus and Sophocles or of the dramas about English kings by Shakespeare. He was able to recite the whole evening and to personify every role, even those of the heroines and the whole chorus, in an amazing and distinguished manner. It was always an exciting pleasure to listen to him.

We also heard lectures on creative arts but most interesting were those by a physicist who experimented with all kinds of apparatus and popularized the natural sciences. Consequently, I boiled a mixture of colophonium, turpentine, and other things to make an electrophore to load an electrical jar. This functioned quite well but in trying to make a bigger one I took too much of the ingredients and suddenly a mighty column of fire came

out of the pot and boiled over onto the table. To avert a fire I took the burning pot and put it under water and burnt both hands so that the doctor had to treat them for six weeks. As a result I could not write and that was quite agreeable. In spite of this the electrophore worked for years.

Except for the theatre and concerts we stayed mostly at home. My father had a passion for playing billiards and bought a table on which we played with great pleasure. We had also inherited our father's talent for pistol shooting, but none of us inherited his enterprising genius for business. I was interested in the machinery but never seriously thought of becoming his successor.

When my father died suddenly in his fifty-eighth year (1879) of pneumonia, I was only fifteen years old and my mother was forty. She was a handsome woman but did not marry again because she took too seriously her obligation to educate her five children in their critical years. Upon his death I saw many tears in the eyes of his older workmen who had also lost a kind friend.

During my school days I read many good books and silly ones as well. Although our work at school covered long hours, I found time to read the German poets and philosophers. Our weekly schedule included nine hours of Latin and three of Greek. Besides, we had to write Latin compositions both in class work and in our examination. The pleasure of reading ancient literature in the original was immense. Beyond that, Latin is a language which trains the mind to say exactly what one wishes to say. It is the most logical language I can imagine. No doubt one can speak illogically in any language, but the Latin grammar is adapted to form sentences easily. Only mathematics and philosophy help you sharpen your logical thinking, but how few people are gifted in that way! In our class of fifty to sixty students there were two real mathematicians.

Except for skating, exercises on gymnastic apparatus, throwing discus and javelin, and wrestling, we did not engage in sports.

Every year three days were very exciting during my school years. One was the second day of September, commemorating the surrender of Sedan in 1870. After the patriotic celebration in school, about five hundred students with their parents and sisters, would get on a special train and go out into the open country to a big restaurant with large tables all ready for us. Garlands and Chinese lanterns made the scene more festive. All the teachers and their families joined us. At first we all sang the Latin song

Gaudeamus igitur, which every boy knew by heart. The *Academic Festival Overture* of Brahms culminates in this music. Brahms wrote the music upon invitation by the University of Heidelberg when it celebrated its fourth centennial in the presence of thousands of foreigners who had at some time studied in Heidelberg. Since themes had slipped into the *Overture* from humorous student songs with texts not exactly decent, many of the old professors shook their heads. It was unbelievably new, but it had a tremendous success and is now recognized as the work of a genius all over the world.

After the singing, athletic games were held for the younger and older boys separately, and fine prizes were distributed. In the evening a dance was held in a big tent, and fireworks completed the day. At nine o'clock, the train took us home while we sang merry student songs. It was the most glorious day of the year.

Another festive day came in spring when every class, under the guidance of one or two teachers, or the gym teacher, took an excursion into the low mountains nearby. Sometimes, two classes were combined. After a train ride of one hour or a little more, we arrived at the point from which we started out on foot with a small knapsack on our backs. After a hike of approximately two hours we could rest, roast our potatoes, and make coffee to drink with our milk and sandwiches. A teacher, or one of the boys, read a story about the ancient history of the spot or about its geology. Then we played some athletic games, sang group songs, and drank coffee or milk. Afterwards, we returned to the station, and (as far as I was concerned) slept like a log during the following night.

The third day of celebration was the one when we gave a public concert. The tickets sold for twelve cents. Every student who played an instrument was a member of the school orchestra, which was conducted by the teacher of singing. He was tall enough, but since there was no podium, he climbed on a chair in order to be more imposing when he beat time. His name was Ahrbeck, but we called him Bülow. He was especially fond of the chorus of selected students which sang polyphonic songs very well. Among the soloists, my older brother Arthur was remarkable. He had an angel-like, pure, high soprano voice. Later his voice changed to tenor. My younger brother Oscar, of course, surpassed all of us, and I had to sing in the selected choir and to play the piano as an accompanist for the instrumentalists. Thus, our Mr. Bülow soon recog-

nized the best part of my musical gifts, my adaptability. Those concert evenings were exciting as well as inspiring.

During vacation time we took a trip to the mountains or to the seaside. It was always the highlight of our school year. At that time, traveling was not much of a pleasure for parents with five children. My father used to travel in a different car from ours. Since a nurse or governess traveled with us, we were seven in one compartment. It certainly was a hard task to keep five lively children for a whole day in one small compartment traveling from Hanover to Munich, for instance. If a stray traveler got into our compartment he could not get out before the next station since there were no passage ways which connected the compartments. The conductor had to get out in any weather to reach the next compartment when he was inspecting and collecting tickets. But we got a good dinner at Bebra. In Munich we would spend two days at a hotel. To get from Munich to Tegernsee we had to travel by stage coach. That was a great pleasure and we all strove to sit next to the coachman, the *postilion,* and tried to blow his horn. I also remember another trip through central Germany when we traveled by coach and had to pay tolls at least four or five times. The money was dropped into a little bag fastened to a long stick which was held out to us through a window. Otherwise the collector would not lift the barrier. This trip took us through Thuringia, where we visited the Wartburg and saw the great hall well known through the opera *Tannhäuser.* In Martin Luther's small study we looked with solemn horror at the inkspot on the wall produced by Luther when he threw his inkpot at the devil. The devil got away but the inkspot did not.

My father had already passed away when one year we went to Norderney, an island in the North Sea. My older brother Arthur and I were allowed to cruise on a fisherman's sloop to Helgoland, then still a possession of England. The sloop was not made for a pleasure trip. It had two masts between which there was barely space for a small life-boat. Our crew consisted of a captain, a helmsman, three sailors, and eight passengers who occupied the deck. Cabins did not exist, nor a kitchen, but we arrived in Helgoland after eight hours, in time for dinner. The next day we bathed and rowed around the island, a mighty rock, with little space for a few narrow streets on the shore near the ocean. On the rocky part of the island the English Governor lived with some guests and

a horse, the only quadruped on the island. Under Kaiser Wilhelm II the island became a German possession in exchange for Zanzibar.

One of the passengers in our party had to go home as soon as possible so we decided to return after dinner. It was nearly seven o'clock when we set sail. For two or three hours we stood or walked on deck, when a thunderstorm broke out. After two hours it was over but it cost us one sailor, who broke an arm as he and the others were lowering the sails, which, buffeted by the storm in every direction, were being torn to shreds, and threatening to kill the whole crew. It also cost us the single bench on which we had been sitting arm in arm. After the sails were down, the lifeboat which was occupied by a passenger and swung between two masts, suddenly threw him out on to the end of the well-fastened bench on which we were sitting. The legs collapsed and four of us slid down from the bench and then crawled on all fours to the middle of the ship in order not to be washed over the rail, which was about one foot high. There we could hold on to the tackle. We were already wet through and with every up and down of the boat we were drenched by wave after wave. Fish and little sea animals were thrown on board and disappeared with the next plunge. Finally the storm abated so that the captain and the helmsman together could bring the boat at right angles to the waves. Then we were not molested by the breakers any more. It was already four o'clock in the morning when the storm stopped. and I went to sleep with my head on the cables. When I suddenly awakened, my face burned like fire. The sun had burnt my wet skin. At nine o'clock we could see the lighthouse of Norderney and we could hear the dinner bell at one o'clock. But how could we reach our island? The wind had died down completely; not even a breeze stirred. We had nothing to eat or drink. But that was not so bad. We tacked slowly to the port when suddenly a new storm broke out and tossed us to and fro.

Since there was no possibility of entering the port, we sailed close to it on the shallow shore and the small life-boat carried us in three trips to a high cart with two horses which we had signaled. It carried us to land where our mother was wringing her hands, having waited for us the whole day. It was then eight o'clock. We answered her hundred excited questions at first with only "It was a great pleasure," and went to our beds which seemed to swing as high as a house; but we slept long and deeply. In the casino we were the heroes of the day and could not exagger-

ate enough the dangers from which we had escaped, to satisfy the curiosity of the ladies. What we did not say was that we felt very tiny in the grandiose tumult of the sea and looked at the sublime spectacle without any fear for our unimportant lives. It was the first time that we were aware of our weakness against the frenzy of nature and it made a lasting impression.

1883 — 1889

I passed the most difficult examination of my life and followed my older brother who studied in Leipzig. Soon he became ill and the most famous clinicians could not make a diagnosis. I brought him home and our home doctor, a new one, made the diagnosis in five minutes. He felt the pulse, looked at the eye grounds, noted increased intracranial pressure and diagnosed a tumor in the right frontal part of the brain. My poor brother died in the twenty-second year of his life; in 1883, no surgeon dared to operate on brain tumor. He was the only one in the family who took completely after the family of my mother, and he was her favorite son.

I then proposed that we spend Christmas all together in Wiesbaden. There we lived in a large hotel with a few other guests and we soon became friendly with one another. One evening mind-readers, quite the vogue at that time, was the topic of conversation. Some persons in the party doubted the possibility and I denied it emphatically. My younger brother contradicted me and offered to serve as a guinea pig. He went out and the company made silly suggestions as to what he should do. I insisted that he should stand on his head on a certain square of the large carpet. He was called into the room wearing a bandage over his eyes and a lady conducted the stumbling young man into the big drawing-room. When he got to the designated spot on the carpet he stopped short, felt his head repeatedly, left the spot, returned to it, and then suddenly stood on his head. The whole affair had been agreed upon by me and my brother in every detail in the afternoon. This is done by every clairvoyant. Clairvoyance would not work without swindle. My family went home and I returned to Leipzig to my beloved skeleton of which I drew all muscle insertions from top to toe.

It was in Leipzig that I met Edvard Grieg in the home of Dr. Abraham, the owner of the world famous Edition Peters. Grieg was a wonder-

fully amiable and modest man who aroused your warm interest in him and his musical intentions. Some years later my younger brother met Grieg in the same house and from the first hour they were friends until the end of Grieg's life. He brought my brother to Munich where he played the Grieg concerto with the famous Kaim orchestra under Grieg's conductorship. My brother also translated some of Grieg's songs into English.

It was disastrous for my brother that my mother did not allow him to study music, because she respected my father's wish that we boys should go to the university. When a schoolboy he wrote music. Twice he was not promoted to the next grade and therefore he had to leave school. Even then, my mother did not give up but engaged a private tutor, a doctor of philosophy, who, after years of worry, resigned and married my younger sister instead. My brother was sent to Hamburg to an exporting firm and from there to London. He became acquainted with some artists with whom he gave joint recitals.

Dr. Abraham was a philanthropist who, among other things, made a gift of a new library, a very fine building together with many books, to the city of Leipzig. Later we spent many joyful days with him in the Harz mountains. He loved young people, but was a bit melancholy and lonesome. He once said money is no blessing for money can buy all you want. He was a bachelor and not a beauty. Thanks to Dr. Abraham, I had an opportunity to hear a rehearsal of a concert under the conductorship of Brahms. Brahms knew how to make clear his intentions but was in bad humor and very critical. It took a long time before the orchestra followed him. Finally, when the chair of the first 'cellist suddenly broke and he fell down, it looked so funny that Brahms could not resist laughing and then he was all right.

In Leipzig I began my studies of anatomy, histology, and embryology with Wilhelm His. He was an excellent teacher and illustrator. In Baltimore, in the Carnegie Institute of Embryology, there is a collection of His's slides, to which I made a modest contribution. I also became acquainted with Thiersch, who was famous as a surgeon in transplantations of the skin and as a pathologist, and also for his caustic wit. King Albert of Saxony wished to be present at an amputation. After having finished the operation in an elegant manner, Thiersch saluted with the saw and asked, "Does Your Majesty wish the other leg also?" Another of my

teachers was the famous physiologist, Ludwig, a very spirited man. Once he demonstrated a frog that jumped when irritated by an instrument. Ludwig then said, "You will see that this reflex disappears when I cut the medulla from the brain." He did so and the frog jumped at least as high as before. The whole audience laughed but Ludwig remained calm, saying, "You see now how little brain is required to amuse a whole group."

During the summer of 1884 I studied at Heidelberg, taking comparative anatomy under Gegenbauer and physics under Bunsen. I do not know which impressed me more, the clarity of Bunsen or the astonishing knowledge of Gegenbauer. I could not study anything else because I had to digest the lessons of these great men. Moreover, Heidelberg in summer was too beautiful for continuous study.

In Baden-Baden, during my vacation, I became engaged to my cousin whom I married six years later. She was three years younger than I, the daughter of my mother's brother, an architect and art collector. My fiancée and I were in no way similar so there was no danger for our descendants, a problem which did not agitate the scientific world very much at that time. Now it became clear to me that I should study seriously. I went to Strassburg, which had a university famous for its teachers after it had become a German town in 1871. It was indeed a wonderful opportunity to study medicine under the anatomist Gustav Schwalbe, a pupil of Ernst Haeckel; the physicist W. Kundt, who experimented infallibly; and the pathologist, von Recklinghausen, the very first assistant of Rudolf Virchow. I learned from him the fundamentals of pathology and the art of dissection and of cutting sections by hand. For staining, we used the natural dye stuffs known at the time, cochenille and carmine. Both were excellent and durable, even up to the present. I was interested in pathology but I did not plan to become a pathologist. To study bacteriology one had to go to the botanist, de Bary, to whose house I was sometimes invited since I knew his brothers, a doctor and a lawyer, in Frankfort-on-the-Main. He was as dry as an American.

I admired F. Hoppe-Seyler for his profound knowledge and his modest character. He was the first to apply the spectral analysis of the blood in cases of intoxication by carbon monoxide gas in 1865 and he wrote the first modern textbook in his field, physiological chemistry. With

two friends I also took a practical course in this field but I must confess that I was not sufficiently interested in it.

The surgeon was Lücke, co-editor with Billroth of a general surgery. Lücke was a genial man who operated brilliantly and antiseptically with phenol sprays and sponges, but not aseptically. After having washed his hands with soap and phenol, he turned up his trousers in order that they would not get bloody and so that dogs would not follow him on the street, as he was wont to say. He also put the knife between his teeth. My friends and I had the private pleasure of discussing theoretically the difference between antisepsis and asepsis of which we had never heard before. Billroth was a famous surgeon in Vienna in the second half of the nineteenth century, who performed the first resection of the oesophagus. He was a good musician and a friend of Brahms and wrote a famous booklet, *Who is Musical?*

The most brilliant teacher was the internist, Adolf Kussmaul, known among the gynecologists for his book *The Malformations of the Uterus.* His diagnoses were not far from infallible so that when he retired in 1887, von Recklinghausen at a banquet could say of him that he had not once been able to reverse a diagnosis after dissecting one of his patients. It was always astonishing how carefully he examined his patients in order to arrive at a diagnosis. When Naunyn, his successor, came to inspect the clinic, he was continuously astonished and asked; "But how can you know that?" He also asked, "Where is the room for the animals?" We had no animals, we had only patients. We had only a wooden stethoscope, a hammer for percussion, the pump for examining the undiluted stomach contents, of which he was the inventor (1869), and the thermometer, of which he would say: "Since we have the thermometer the doctors have forgotten how to diagnose by feeling the pulse." Before he came to the University he had been a country doctor traveling on horseback. He once said at table to the five students who used to meet socially in his home, "The best opportunity for young doctors to learn something worthwhile is to have a country practice." This impressed me so deeply that I decided to become a country doctor for some years instead of a Fellow.

During the last year of my training I had to study internal medicine with Naunyn, co-editor with Klebs and Schmiedeberg of the *Archives for Experimental Pathology* (1873). It is not easy to understand how deeply we were shaken by the difference between these two great inter-

nists. Kaussmaul, whose therapy was based only on the experience of many generations, and Naunyn, who would not apply drugs if they were not tested on animals and chemically known. All the accepted and proven decoctions and infusions and pills were rejected and it was always a shock when, at the end of a case presentation, the medication was torn to pieces by criticism. We asked ourselves why we should study materia medica and were very confused. In truth, Naunyn was not as radical as many others. He recognized the influence of some of the old medicaments and, in a case of scurvy, he recommended yeast, to the greatest astonishment of his audience. He said, "It is true we do not know how that works but it helps." In this "therapeutic nihilism" he agreed with our pharmacologist, Professor Schmiedeberg, who once would not approve of castor oil as a laxative because the rabbits did not react to it. Kussmaul answered, "If your rabbits do not react to castor oil, I am sorry for them; my patients always react promptly." However, he applied a good old Swabian word for "react," for which I find no English term in Webster.

The gynecologist and obstetrician in Strassburg was Wilhelm Alexander Freund, who is known especially because he performed the first laparotomy for cancer of the corpus uteri (1878) on a patient whom he could show eighteen years later at a meeting of the German Gynecological Society in Breslau. But he was not proud of his success. When he retired from Strassburg to Berlin, where I visited him often, he regretted that he was famous for that operation rather than for his fine pathological work in the parametrial inflammatory diseases; he was an idealist who considered this work to be of higher value because of the greater amount of diligence and study he had applied to it. His large clear eyes brightened his whole audience and his vivacity forced them to listen. He had read Hippocrates and Galen in the original and he quoted from the history of his field on many occasions. He was a beloved teacher who knew how to make all things interesting. He played the violin and the viola very well and he had a wonderful quartet with his son Wilhelm, W. His Jr., and Lindemann.

I cannot dismiss Strassburg without mentioning Heinrich Bayer, who taught us theoretical obstetrics. He is known especially for his textbook on the pelvis and on the development of the female sex organs. He was the son of poor parents and was an autodidact until somebody made it

possible for him to study medicine. He used to speak and write the most elegant German.

My first day in Strassburg coincided with the anniversary of the University. We made an excursion to the Vosges. I joined a freshman, Curt Neugebauer, a boy of my age, and he and I have been close friends from that day to the present. He advised me well in making out a study program and told me which lectures to attend. He brought books from the library which he thought might interest us. We were alike in our philosophy and in our artistic interests. Since he was a good pianist, we played four-handed or he accompanied my violin or my singing. This boy later had a practice in Fraulautern, which is near Saarlouis. The city is now destroyed.

It was also in Strassburg that I fulfilled my military service in the year 1886; it was for me, an agreeable interruption of my studies and ended after six months with a fracture of a malleolus. I never felt as well as during that time in the army which was indeed strenuous but taught me to respond instantly to commands. I learned to jump much better with my injured foot than with the other and I received a pension of nine marks monthly for about twenty years.

In the years 1887 and 1888 I twice guided a party of four students on a tour of Italy, since I spoke Italian. We paid one hundred marks, about $24.00, and enjoyed the trip like kings. We came to know especially the big lakes, Como, Lugano, and Maggiore and the towns of Milan, Genoa, and Verona. Verona is a typical Italian town with an ancient amphitheatre and monuments of the Colleonis. Genoa is less interesting and its finest place is its cemetery with monuments of great men. Milan, apart from its cathedral and the Galleria Vittorio Emanuele, is a town which would fit into any country. It is not a true Italian city. The people, including babies, appear at ten o'clock to eat ice cream in quantities, just as in America, but not in drugstores. Ice cream is consumed on promenades.

In Strassburg I was the leader of the tenor section in the choir of the Academy of Music. One day I was ordered to come to the director, Professor Stockhausen, who immediately put me in a small room between himself and the orchestra to sing the second tenor part of a double quartet from Mendelssohn's oratorio *Elias,* which I had never heard before. The noise around me was frightful but I passed the test; the director gave me

three lessons and I sang in the double quartet together with professional singers of the first rank. You can imagine the astonishment of some of my professors and co-students.

By the way, there were many pretty girls in Strassburg who would have liked to study at the university but were not permitted to do so. They had to go to Zürich in Switzerland. In Zürich at that time there was a physiologist who told his mixed audience: "Now we have non-alcoholic beer and wine; we have nicotine-free cigars and cigarettes, and we have coffee without caffein. If we can have unattractive girls our happiness will be complete."

Prominent among the students was Ludwig Aschoff, later the famous pathologist. On account of his joyful temperament and his brilliant eloquence, he played a leading role among the students. In contrast, I was a little shy and shunned publicity. Perhaps we therefore felt a liking for one another, but our really friendly relations began later when he was already Professor of Pathology in Freiburg. There was almost no topic which he could not discuss in the German Pathological Society. He was early in life a very famous man. He was an extraordinarily gifted scholar who had time even for politics, and he was an excellent teacher who attracted many scholars from other countries, especially from Japan, who came to work under his special direction. I still have a small bronze relief of him, given to him by his friends on his seventieth birthday.

There was another prominent student, August Wassermann (later von Wassermann), a small modest man with clear friendly eyes, who wrote his thesis for the doctorate on the effect of sulfonal with my help, inasmuch as I was one of his experimental rabbits. Sulfonal was detected in 1884 as the first of the modern sedatives. Later on I seldom saw him.

A third student was Wilhelm His Jr., principally know as the discoverer of the neuromuscular atrioventricular system (1893) with whom I was on good terms. With Dr. Lindemann as 'cellist, we played a trio, he the violin and I the piano. We played also "Skat," the very popular card game, and were a merry company.

Another gifted boy was Robert Müllerheim who later demonstrated the first roentgenogram of the living child in the uterus. He also wrote *Childbed in Art,* illustrated with many reproductions of old pictures. He was for some years an excellent assistant of W. A. Freund, from whom

he acquired a love of art, the ancient classics, and etymology. When he came to Berlin as a specialist we had become good friends. No matter how often I visited him, I never left without having learned something from his hobbies. I mention him especially because he was the third in the group with Neugebauer and myself, to appear before the examiners. The examination presented no difficulties for us although in anatomy it lasted a whole week, and included comparative anatomy, embryology, and topographical anatomy, with a typical operation like that of Chopart. Whoever did not pass this first examination was suspended for half a year and could not go on. The same was true for the second field, physiology; but in the other fields it was not so severe.

At that time we were trained more as scientists than as general practitioners, but we also learned to be independent doctors. Later on, young men had to spend one year either in a hospital or as assistants to a good doctor who was licensed.

The dean, Hoppe-Seyler, after having examined us in hygiene, told us that we had passed the examination and congratulated us solemnly. That moment I shall never forget. We never before had realized that now our gay life as students was to be at an end and that we should now go in different directions and learn how to be on our own. It was December 29, 1888. What now? That was the only thing we had in mind when we left one another with dismal faces. Soon afterwards we wrote our theses for the doctorate and then we had to part from one another. I went to Berlin to study as a volunteer worker at the Krankenhaus am Friedrichshain, especially to learn the finer diagnosis of the diseases of the urinary tract from Professor Fürbringer who had written an excellent textbook about his speciality. It was he who introduced the alcohol-sublimate disinfection of the hands before operations (1888). I learned very much from him. On the other hand, I could correct their diagnoses of the diseases of the nervous system in which I had become especially interested through Kussmaul. For instance, a diagnosis of myelitis transversa in a woman had to be changed to simulation. No one would believe me but a few days later the police came to take the woman to prison, from which she had escaped, a quite healthy person. After that I was consulted for all nerve diseases. I remained there some months and used the rest of the year 1889 to learn the new techniques in various fields for my country practice.

In this year I became acquainted with Robert Koch in the Community Hospital of Moabit where he tested his tuberculin on patients. He was, at that time, a very serious man who did not speak much. The next year (1890) he was urged by the government to publish his work, although his experience was as yet not large enough. There existed a silly rivalry between Germany and France as to who was the greater scientist, Koch or Pasteur: they should have erected a common monument to both men.

1890 — 1896

In December 1889 my mother sent me a press clipping about a country doctor who desired to give up his practice, in a place of 2100 inhabitants, with a railway station and including some minor villages. The conditions of taking over his house, three horses, buggies, and a sleigh were agreeable. My colleague there, Dr. Handtmann, who had been, for some years a fellow at the university in Halle under Professor Weber, was an old practitioner, not at all affected by modern nihilism. Dr. Handtmann had learned a great deal from him and gave me prescriptions for many disease. One had to learn the right dosage for every age and constitution.

I took that practice with house and horses the day I saw it; I married and moved in with my young wife and furniture one month later. The place was called Dedeleben in the province of Saxony. I found an agreeable population and some very good friends, mainly sugar manufacturers, two pastors in our village, and doctors in the neighboring villages, and two pharmacists, of whom one played the flute and the other also was a bit silly. Soon I was on the best terms with nearly all the people in the village and also with the two doctors who lived in two villages in opposite directions from Dedeleben. They were enemies because each had the better horses. Once, one of them came to show me his new horse, which he had bought from a gipsy for one hundred marks and his old horse. I took one look at the new horse and said: "Quite nice, but that is the horse which Dr. Klavehn (his enemy) sold to the gipsy because it limps quite often and he also paid one hundred marks for his new horse and gave away his old horse besides." And the horse really limped after a week.

On another occasion the other doctor came to show me his new one-horse carriage. The horse went like the devil but looked like a camel.

It had won third prize in a long distance race between Berlin and Vienna. To the small light carriage a back seat was attached by two screws only, for the coachman whom the doctor lost after a week, just as I had predicted. The man fell down on the stone pavement and had to carry home his seat over a distance of one hour's walk. He did not find the screws, however.

My practice was not easy. I operated assisted by a younger colleague. I delivered children and was busy almost all the time. It was in 1889-1890 that we had the first heavy influenza epidemic in Europe, which kept me extremely busy; it recurred in winter, while in summer one could recuperate. In summer the farmers had no time to consult a doctor. One of the two mid-wives showed me how she delivered herself of her thirteenth child. She did it in an astonishingly short time by kneeling in her bed, and very skilfully, too. The other mid-wife, who was childless, furnished her four rooms on the second floor of her home for my surgical patients. She was exceptionally clean. We boiled the instruments but had no facilities for sterilizing the bandages. They were sterilized at the factory but the secret of our brilliant cures was that there were practically no streptococci. We had not one single case of childbed fever, nor a single case of infected wounds after an operation. Some few cases of paronychia or otitis media after scarlatina healed promptly under wet bandages. Only seldom was I forced to open the mastoid process among hundreds of cases. The infectious diseases migrate from house to house.

In my village lived a pastor whose son suffered from incurable weakness of the heart. I therefore came to his house often. The father, although more than sixty years old, had to learn his sermon by heart every Saturday from a book of sermons which he had written himself. One could see him in his garden and hear him recite with a loud voice while walking around. He had a tremendous thundering voice and spoke with great pathos even in daily life. With this voice he would greet, for instance, another pastor: "Now, brother in Christ, tell me how the new cigar has turned out!" In addition, he spoke a terrible dialect, that of the people in Brunswick. This clergyman once asked me to attend divine service together with my wife and to sit in the pew of the Baron, "so that all the people can see you. You know that people nowadays do not go to church as often as in old times. You would set a good example for the people." Thus we entered that reserved pew through a special door near the sheep

stable, on Whitsunday in company of an old landlady. At first we behaved all right. But then the pastor paused solemnly after he had said in his Brunswickish sermon: "And the voice of God speaks through me," and at that moment the sheep began to bleat in the purest Brunswick dialect. Mrs. Meyer almost swallowed her handkerchief and I disappeared in a great hurry stumbling over the old winding staircase. It was a shame; we decided "Never again." We were so young and the weather outside was so beautiful, really it was a shame,

One of the well-to-do farmers was always worrying, especially about himself. His wife had no children, a circumstance which the country folk considered to be an unkindness of his neighbors. Once he made me come to his house in the middle of the night because he found something in his urine which he could not explain to himself. It was wax which had dripped from the candle and had congealed into small balls. This good man was the local authority on all questions of policy. When the epidemic of cholera asiatica was raging in Hamburg in 1892, the government ordered that every community provide a small hospital and take precautionary measures. That farmer called a meeting of the important men of the village, which took place in an inn. Whenever the word cholera was mentioned he trembled. I made certain proposals which were accepted when suddenly the rural policeman came in and reported that somebody was groaning terribly in the out-house of the inn. The authority turned pale as if he had come down with the cholera himself. We went into the courtyard. I was the first and the local authority the last. Really, somebody was groaning pitifully. I knocked at the door and asked: "Have you diarrhea?" "No, on the contrary" was the answer. That ended the first and last meeting; it could have happened anywhere during the cholera epidemic.

One has to take care not to get a reputation as a performer of miracles by giving prognoses that are too detailed. I found that out. For instance, there was the case of a boy who had fractured his skull. The boy was cured after three months. Half a year later the father told me of their astonishment at my exact prognosis, even on the first day, down to the days and hours. I did prognose in detail so as to warn the family what to do and what not to do, but I certainly did not prognose for every hour. Yet people in their superstition falsify their reports afterwards. I cannot

omit another case because its end was tragic and the tragedy was perhaps partially caused by me.

One afternoon when I entered the inn (there were no other guests) the innkeeper greeted me with the words: "Now, doctor, my turn has come." "What do you mean?" "Do you not remember, it is now three months ago that the three of us asked you what age old Knacksted was who died that day? When you said eighty-four years, we replied that none of us would get that old. You answered quite calmly: 'Not all three together.' Now, it is my turn." The two others had been twenty-six and twenty-seven years old and were both known as habitual drunkards. Four weeks earlier the first man had died of pneumonia and delirium tremens. That day the second man had passed away on account of a heart attack. "Now it is my turn," the innkeeper repeated and drank a glass of liquor. I tried to calm him and examined him. He had an extremely dilated heart but otherwise he was a strong man and had never been ill. I told him he could become very old if he stopped drinking. I said that only to warn him; there still were a number of years left for him, but he insisted it was his turn now. A week later I was called to the inn at midnight where I found him in an advanced stage of delirium, completely unconscious and surrounded by his wife and parents. His heart was extremely weak and stopped often enough to make me fear the worst. Therefore I stayed with him until he woke up after a few hours. He spoke quite clearly and begged that his family forgive him. He asked me to do the same because he had not listened to me and then he passed away. It was a dreadful night for me; my conscience was not clear. The people, however, would say: "See, the doctor told them in advance that they had to die, all three of them. Isn't he an extraordinary doctor?" The patient of whom I could boast that I prolonged her life until she was more than one hundred and seven years old, passed away without any illness, blown down like a leaf, after I had been in Dedeleben for half a year.

After two years in Dedeleben we were expecting our first child. One evening we encountered the blacksmith, who said, "Well, I wish you the best since one always stands with one foot in the grave." This encouraged my wife mightily so that the delivery next day came to a good end. And it was a son, of whom we had spoken so often. He developed into a charming child and he wished to be a musician, which we noted as early as his second year—believe it or not!

We enjoyed our sojourn in Dedeleben and surroundings indeed. We made excursions to the Harz mountains and visited, on the way, some patients of mine on country estates, where we were welcomed as good friends. We also had medical evenings once a month in Halberstadt, a small city of 25,000 inhabitants with a good theatre and with modern specialists of all kinds. Prominent among them was the surgeon Hans Kehr, a gifted man who was developing as a specialist in gallbladder surgery, with the country doctor's help in sending him patients. He was later a famous man in Berlin, after having performed more than two thousand operations; he wrote two large volumes on his methods of bile duct surgery. Even in Halberstadt, his income was such that he engaged singers of the first rank to render his favorite Wagnerian operas and entertained them as his personal guests.

Only once did I abandon my political aloofness; when "Social Democracy" began to conquer the country I opposed their speaker, a very eloquent man. I should have forgotten the incident if an unforgettable episode had not happened on the same day. I was crossing the courtyard of the estate together with the manager, when we saw the swineherd with a ballot in his hand. The inspector asked him what he had there and saw the name of a Social Democratic leader on the piece of paper. "Don't you know that that party will ruin the farmers altogether? How dare you support such a dirty party?" He tore the paper to pieces and handed him the liberal ticket instead. "This you turn in." "I shall do that," said the swineherd. "Do you have more of those?" "Yes, they are up in my room." "You must promise me to tear them to pieces." "I'll do that." In the afternoon the inspector met him again. "Did you turn in the ticket I gave you?" "Yes, sir." "And what did you do with the others?" The man answered with the slyest expression: "I fooled the cowherd with one and I tore up the others." This story was published later in a comic magazine but it originated in Dedeleben.

Otherwise Dedeleben had rather an intelligent population and there were even some very rich farmers. The soil was suitable for sugar beet culture and when I came sugar beets had already been grown for two or three generations. Before the sugar beet was known all our sugar had been imported from India in the form of big cones, called "sugar hats," which had to be crushed into irregular pieces. When the sugar contents of the beet root became known farmers started to send their sons to the

agricultural school in Halle on the Saale River, situated in the same province, where the cultivation of sugar beets was advertised. A number of these sons had a high school education and thus became reserve officers, a position of importance in Prussia.

They had modern stables for their cattle, with running drinking water covered with lids. One of the cows in the village loved me and showed it by soulful lowing. I do not know why she should have loved me but I was polite enough to return her love. She lowed until I stroked her and said some kind words to her like "Hello, old animal, how are you today?" Then she appeared contented for the whole day. If only all love could be satisfied so easily! One day I thought I would demonstrate the cow's love to my son who then was one year old. When he heard the lowing he himself began to low as loud as he could so I had to take him out of the stable. The cow misunderstood this and never lowed again when I entered the stable. My son, for his part, climbed over the railing of his bed during the night, came into my bed and cried: "The moo-moo is coming. The moo-moo is coming." This went on for about a week until the incident was forgotten.

One day I was crossing the market square at Dedeleben where a merry-go-round, a miniature rifle range and similar curiosities had established themselves over night, when a dirty little dog came and greeted me in a very friendly manner as if he knew me. My mind was preoccupied with a patient and the dog ran away. The next day the dog greeted me in so friendly a way that I took a look at it and recognized it as our own former dog. "Tilly," I exclaimed, and the little dog was so happy that she came along with me without being invited. She had not seen me for nearly two years. We had had to give her away because she licked our child and had a tape-worm. When we arrived in the kitchen the cook was just cleaning the floor and she exclaimed "What is that dirty dog doing here?" But when the dog insisted on carressing her she suddenly exclaimed "Tilly!" The same thing happened when my wife saw the dog. In spite of her dirtiness we still loved Tilly. At dinner time she came to the room, took her seat on the middle one of three chairs just as two years before. She waited until we had finished and then received her food by the stairway. In every respect she acted as if she had never been away. For instance, when it got dark she went downstairs to the basement. We brought down her basket but she did not go to sleep and came up three

times until I remembered that she always had a little bowl of water close by the basket. When I brought that to her she was content and slept the whole night. Punctually at seven in the morning she came to our door on the second floor and scratched till we opened. After a touching greeting she went to the door to the balcony, as she had always done, to see what was going on outside. She got her milk and her bread and at nine o'clock she disappeared never to be seen again.

When our son was two years and eight months old we thought that he would soon have to go to school and it would be better not to wait any longer but to leave Dedeleben before getting too old. I was then thirty years of age. Again it was my mother who did not wish us to stay there and who played a providential role in sending us a press clipping in which a doctor asked for a country practice. He came at once and on the first day took my house and horses and vehicles for the same price I had paid for them four and three-quarters years before. Eight days later I was in Berlin with my wife and son and furniture in a new home which an uncle had procured for us. "Physician and Obstetrician" was printed on the door of the house. The latter had the first patient.

We had been in Berlin just one year when a daughter arrived who happened to have a mighty voice, so we decided she should be an opera singer. And, after the war was over she really became one. Unfortunately, on her childbed, my wife had severe pain from gall stones, which recurred about monthly, so that in the spring of 1896 she accompanied her father to Karlsbad in Czechoslovakia, an internationally known health resort. She had gone from Karlsbad to her father's home in Frankfurt-on-the-Main when suddenly a telegram called me to her because she was seriously ill. The same day, with the two children and a nurse, I took the train for Frankfurt. She had a large empyema of the gallbladder. Prof. Rehn operated on her the next day and she recovered, but had a gallbladder fistula for about a year afterward, when our friend Hans Kehr operated. Kehr was then still in Halberstadt. He found another large stone and the fistula was cured. I give this history because the predisposition to this illness was observed in four generations of females. My wife's grandmother had a gallbladder full of stones which did not cause her pain since the gallbladder was occluded. The mother of my wife died of gallbladder stones which caused an acute cholangiitis. My wife and our daughter were operated on and one daughter of my wife's sister also.

In Frankfurt my wife was nursed by a Sister Edmina who was a nun, a distinguished young lady like many of her calling. She came from a good family in the Rhineland. She had such affection for my wife that an exception was made and she was permitted to take care of my wife when we were in Halberstadt, outside of her conventional province. Nobody could understand what this meant to my wife unless he knew her temperament. They carried on a life-long correspondence and a year and some months ago I received indirectly a letter from Sister Edmina who had become Mother Superior of her convent. The letter was addressed to my wife inquiring how she was.

Now, back to 1895. I met a doctor in the home of my uncle, who asked me incidentally whether I had enough to do or if I would like to volunteer in the private clinic of a gynecologist, Professor Veit, who was at the same time Professor at the university. This man was the first to diagnose tubal pregnancies and to call attention in a monograph to the symptoms of extra-uterine pregnancy. He had also formerly worked with Carl Ruge in pathology, as we shall see later. He was the editor of the *Handbook of Gynecology* (1897), published after his death as *Veit-Stoeckel's Handbook,* 3rd Edition. I helped J. Veit with his *Handbook* by contributing two publications, dealing with the etiology of gynatresia and the hematokolpos in double uterus — my first publications in this field (1896, 1897).

Veit was a genial man and he emphasized it a bit. He had a private clinic in which he operated not too aseptically, with the help of two assistants and myself, and also an out-patient practice. Besides, I had the pathological laboratory, not much larger than a closet, and that was the beginning of my career as a pathologist. I furnished one room of our overly large apartment as a laboratory because I could not sit far from home day and night cutting sections in case patients should come—and they did come.

I had a microscope, bought a small microtome, and learned in a short time to make sections and diagnoses with the help of Veit and books. Soon I was a virtuoso who could make sections through a whole kidney in celloidin, some of which I still have. I learned to bring the unstained sections from the knife into diluted alcohol onto the slide and to observe them with low power in comparing them with stained sections under high power. That technique I needed in order to look for anomalies

in the uterus of many fetuses and adults. I stained and preserved only the positive findings. It required the patience and perseverance of a mule, which I had. After years of work I needed help. It is to the credit of my wife to have recognized that this help could be found in a servant, a clever and ambitious girl, who learned the technique in a short time. She was with me for thirty years and taught generations of technical assistants. During the last eight years of her life she cut nothing but serial sections of more than a hundred and thirty fetuses for my work on the development of the vagina. She died tragically in about 1936, a victim of Hitlerism. She was Catholic and had an illegitimate son by a Jewish pharmacist, who generously supported her so that she could educate her son and make it possible for him to study veterinary medicine. He received a steady appointment as assistant to a veterinary surgeon who concealed the secret from everybody. But, on the same floor where his mother lived, somebody resided who knew the secret and tormented the mother with letters and threats until she committed suicide, a fruit of the teachings of Hitler who succeeded in bringing out the worst traits in human behavior.

About 1895 there was an officer, Lt. de la Roi, in Berlin who was well known as a balloonist, and whom I met socially. A few days later I flew with him and two other gentlemen. We were in a basket which was just big enough for four persons and which hung by ropes from the large gas-filled balloon. It was the biggest thrill that I had for many years. To chart the course I was to study the map, but during the first few minutes a strong wind drove us in the direction of a huge iron roof decoration of the gas works. This was so exciting that I lost our bearings on which of the many hundreds of railway lines around Berlin we had passed. The resolute throwing out of a bag of sand by our leader saved us from collision. Nobody was able to determine our direction.

We had left Berlin far behind when suddenly one of the gentlemen recognized a building which he had seen the day before in an illustrated newspaper. It was a sanatorium called Birkenwalde, and we were saved—three hours later we would have landed in the Baltic, as had happened to some other tourists earlier. Otherwise it was a wonderful trip in the sunshine since one does not feel the strongest wind. When we had to go down the outlet for letting out the gas failed to open fully and the half-filled balloon rushed with great speed over the ground, the car behind it

tilted over, and while hanging onto the ropes we tried to anticipate what would happen. I remember seeing a hare that ran away as fast as possible. Finally, our balloon stopped on an embankment and we rolled out together. Then we noticed the wind for the first time. Now we had to pack our balloon preparatory to return by rail. One of the many country men who had never seen a balloon asked me: "Is it true that you will return to Berlin this afternoon?" I answered: "Yes, indeed." "But how do you blow up the balloon?" he queried. A fast railway train brought us home taking just the same amount of time we had been in the balloon.

The next morning the adventure was described in the newspaper and all day the telephone rang. That was less agreeable. One of my friends, after he had called me, ran excitedly through Berlin to tell his friends that he knew a man who had been in balloon. Much more important, if true, is a story which people told me at that time about an event in a little village in Bavaria where the people are Catholic. Suddenly a chair fell through the clouds into the midst of the country-side among people, cows, oxen, and sheep. After a little while the animals occupied themselves with eating but the people built a chapel around the chair. I do not vouch for the truth of the story.

During that period I was friendly with Wehnelt, a theoretical physicist at the university. He was then a young bachelor and we took bicycle trips together on Sundays in the Grunewald woods, that are more famous than beautiful. We went to Schlachtensee where my wife joined us in the afternoon, traveling by train. We had mutual friends there, the Donaths. Old Donath, a patient of mine, was an original man filled with ideas for the improvement of the city of Berlin. But the community did not appreciate his project since it asked for the expropriation of residents in the best district of Berlin to the amount of eighty million marks. That was long before I met him. At the time of our acquaintance it would have cost eight hundred million. He was a far-sighted man who built railways in the surroundings of Berlin and one straight through Berlin to connect it with the settlement in Grunewald. At that time brown coal was used, a circumstance that should not necessarily cause that bad odor which prevailed. Bismarck made the first trip on the railroad when all of a sudden the engine began to give out a horrible smell. Bismarck turned up his nose, but Donath, who accompanied him, said: "Your Excellency, this comes from the "Black Ditch." The "Black Ditch" was a gutter filled

with stagnant water which crossed the settlement. "Hasn't that been removed yet?" replied Bismarck. "No, the various communities cannot come to terms about finances." The next day a commission appeared on the scene to settle the controversy. Soon the sore spot disappeared and gave way to a wonderful running creek along which the finest residences were built. All because the soft coal smelt at just the right time. But it also smelt at the wrong time as my neighbors and I can affirm, who lived on the main streets of Berlin West, until the railway was removed in 1899 and the electrically running subway was installed.

Young Donath was a character, but in a different way. He was a physicist too and skilfully popularized physics in his lectures at the Urania, a theatre and museum for physical instruments for the demonstration of the laws of physics. Wehnelt was the inventor of the "Wehnelt interruptor" for x rays. It made a deafening noise splitting water into oxygen, as far as I remember. Dependent on the intensity of the electric current which ran through the water the noise increased and with it the pitch of sound which I had to determine. Most people who did not have as good an ear as I thought the performance was astonishing. I gave a great deal of time to Wehnelt. It was my only opportunity to exercise my talent of pitch for scientific purposes. But I am not proud of this ability since I know that dogs have it too. Otherwise we were a merry trio of gymnasts on those Sundays at Donath's. Our thirst was great and the old lady prepared an excellent punch of light wine with fruit. These pleasures came to an end when Wehnelt was married, old Donath passed away, and young Donath married Wehnelt's sister. I have forgotten the dates and sequence of these events.

(*To be continued*)

Paracelsan Medicine in England:

The First Thirty Years (ca. 1570-1600)

PAUL H. KOCHER*

OF THE TWO great movements, Copernicanism and Paracelsism, which stirred the stagnant waters of Elizabethan science, Paracelsism has perhaps the lesser eventual importance but certainly not the lesser interest as an exhibition of how new ideas penetrate, and of the antagonism, evasion, compromise and gradual acceptance which mark their course. Both movements, springing almost simultaneously on the continent from neo-Platonic origins, achieved a large degree of adoption among English scientists by the year 1600. Both aroused religious as well as scientific controversy. There, however, the parallel ends. Whereas Copernicanism was a well defined and easily identifiable body of philosophic and mathematical doctrine uttered in a single master publication, the *De revolutionibus,* in 1543, Paracelsism was an immense amorphous mass of theological, metaphysical, psychological, medical, and chemical theory expressed in scores of different manuscripts, most of them unpublished during the lifetime of Paracelsus himself and wandering dubiously over the face of Europe for many years afterwards. These manuscripts, some of them genuine, others spurious, were published individually here and there by disciples of Paracelsus after his death in 1541, but no attempt at a collection was made until Huser's edition of 1589-1591.[1] Furthermore, since a number of the ideas basic in Paracelsus, especially in medicine and chemistry, were already rather widely in vogue, the difficulty of tracing specific Paracelsan influence in these fields becomes severe. The present essay makes no pretension to a detailed, technical command of the vast corpus of Paracelsan medicine. Since, however, no survey of the initial impact of this medicine on sixteenth-century England seems ever to have been written, it will be useful to chart the salient issues and their major consequences, which are clearly visible to any painstaking observer.

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[1] Lynn Thorndike, *History of Magic and Experimental Science.* (New York, Columbia University Press 1941) 5. chap. 29. The edition of Paracelsus' works used in preparing the present essay is *Opera omnia medico-chemico-chirurgica* (2 vols., Geneva 1658).

One of the great contributions of Paracelsus was to stress the role of chemistry in medicine. But we should understand at the outset that he did not originate this idea. As early as the days of Pliny the Elder and Dioscorides the procedures for distilling vegetable substances were known, and some metals and mineral compounds were subjected to chemical preparation, usually for external application as ointments and plasters only. These gains were extended by medieval physicians like Arnald of Villanova, pseudo-Geber, and Lully, whose alchemical experiments brought to light, as byproducts, new compounds useful in medicine. They also announced the theory, later developed by Paracelsus, that all matter is composed of a triad of ultimate chemical principles, salt, sulphur, and mercury. Then during the Renaissance a strong interest in the therapeutic potentialities of chemistry was afoot under the leadership of such men as Hieronymus Brunschwig and Conrad Gesner, quite independently of Paracelsus. The function of Paracelsus was less to introduce completely new prescriptions, though in some measure he did this too,[2] than by bitter, full-scale assault to help shake the general faith in Galenic herbal polypharmacy and to push the trend of research towards a more complex and better informed inorganic chemistry as part of the equipment of the medical man. I emphasize this phase of his work because it is the one which was most significant in England. His surgical methods likewise were fruitful there. But his more recondite theories of physiology and pathology had a mistiness which did not endear them to the practical English mind and fell on barren ground.

PRE-PARACELSAN PHARMACOLOGY IN ENGLAND

We need first some understanding of the situation in England before the impulse of Paracelsus began to be felt there about the year 1570. The native alchemical tradition had been strong. The great name of Roger Bacon still so flourished in the sixteenth century that under it could

[2] Good accounts in English of the genuine innovations of Paracelsus in chemistry and medicine can be found in John M. Stillman, *Paracelsus* (Chicago 1920) and Fielding H. Garrison, *An Introduction to the History of Medicine* (Philadelphia 1929) 4th ed., 204-207. Garrison's summary is that Paracelsus made opium, mercury, lead, sulphur, iron, arsenic, copper sulphate, and potassium sulphate a permanent part of the pharmacopeia. For the earlier knowledge of chemistry discussed here, see James C. Brown, *A History of Chemistry* (2nd ed., London 1920), especially pp. 39 and 129-30; John M. Stillman, *The Story of Early Chemistry* (New York 1929); especially pp. 38-55 and 290-93; and John Read, *Prelude to Chemistry* (New York 1937) 26-27.

profitably be printed three books, one on "the beste waters artyfycialles" (1530?), one on retarding old age (1590), and a third on alchemy (1597). During the fifteenth century George Ripley, Thomas Norton, and Robert Grene had written the usual esoteric allegories on the philosopher's stone, and during the sixteenth the sons of art included such adepts as Edward Cradock and John Dastin, whose obscurities were later preserved in Elias Ashmole's *Theatrum chemicum britannicum* (1652). John Dee, himself an experimenter in the occult, owned scores of such manuscripts both English and foreign.[3] Their value to medicine was simply the indirect one of publicizing the potentialities of chemistry and familiarizing readers with various types of apparatus.

More important were the English translations of continental books which applied chemistry directly to medicine. As early as 1527 a translation of Hieronymus Brunschwig's *Liber distillandi,* originally published at Strassburg in 1500, appeared in England under the title *The Vertuose Boke of Distyllacion.* Here was no chemistry for chemistry's sake. Its object was to describe many kinds of waters distilled from herbs, beasts, insects, and even worms, which the poor people could better afford as medicines than the expensive herbal compounds then in use. Woodcuts of several crude types of stills and filters accompanied the text. But perhaps the most interesting feature was Brunschwig's view, which he inherited from the earlier alchemists, that distilled medicines are purer, more efficacious than medicines not thus subjected to purification by fire. Distillation, he says, "is none other thynge but puryfyeng of the grosse from the subtyll & the subtyll from the grosse" so that the subtilized medicine may more swiftly make its way through the human body, where as "when ony medycyne is mynystered with her corpus or substance in the maner of electuaryes, confeccyons, powders or syropys" it makes the body feeble and weak.[4] This was exactly the claim later made so

[3] See J. O. Halliwell, *The Private Diary of Dr. John Dee* (London 1842) 65 ff. for a list of manuscripts in Dee's library at Mortlake in 1583. The fact that of several hundred manuscripts, many of them from the earlier chemists, only one was from Paracelsus (described by Dee "Non est Ramundi Lulii, sed collectanea diversa ex Paracelso") is surprising. I have not met with any reference to Paracelsus in Dee's printed works, but there is one in the *Diary* under date of July 31, 1590: "I gave Mr. Richard Candish the copy of Paracelsis twelve lettres, written in French with my own hand." By and large, Dee does not seem to have been greatly concerned with Paracelsus.

[4] Sig. A1^{r}. Brunschwig's *Homish Apothecarye,* translated into English in 1561 contains nothing of interest in this connection. The popularity of his *Vertuose Boke of Distyllacion* is evidenced by its going through three editions, 1527, 1528(?), 1530(?).

often and so vociferously by Paracelsans and rebuffed by the Galenists. It was indeed one of the main issues of the whole controversy in England. Yet Paracelsus was only seven years old when Brunschwig wrote this book.

English interest in chemical medicines was further attested and reinforced by *The Treasure of Euonymus* (1559), a translation from the *De secretis remediis* (1552) of Conrad Gesner. Resembling Brunschwig's book in general intention, it nevertheless gave a far more advanced treatment of the subject, with some elaborate descriptions of chemical procedure and many detailed woodcuts. Gesner's preface traced the history of chemistry in its relations with medicine back to the late Greeks,[5] and restated the thesis that by such procedures "al the vertue and faculti or operation is separated from the substance of the medicins, so that ye more liquid and moyst, the more pure and subtil part of euery remedy or medicin, maye be had seuered and drawn out from the grosse and erthy part."[6] The reason, said Gesner, why little store was yet popularly set upon chemical remedies was "the lacke of knowledge, Couetousnes or negligence of Phisitions and commun Apothecaries. . . ." Unlike Brunschwig, he gave considerable attention to the manner of preparing quintessences and oils from lead, gold, antimony, vitriol and other inorganic substances.[7] Paracelsus had died in 1541, but Gesner never mentioned him or showed in this book any cognizance of his very similar work in the same directions.[8] Rather, Gesner's writing was a reflection of a concern with the medical possibilities of chemistry rising all over Europe, and especially in the mining centers of Germany, quite independently of Paracelsus. It may be surmised that Paracelsus' manuscripts would never have been published in quantity as began to happen on the continent through men like Gerard Dorn, Adam von Bodenstein, and Leonhard Thurneisser in the late 1560's, had not an extensive public interest already existed to give them countenance. What then occurred was the familiar

[5] Sig. A1^{r}. ff.

[6] Sig. B1^{r}.

[7] See the section "Of oyles of metals" beginning on p. 290.

[8] Gesner knew vaguely of Paracelsus' work and life but had no high opinion of either. Read Gesner's statement printed by Thomas Erastus, *Disputationum de medicina nova P. Paracelsi,* (Lyons 1572) at the end of pars prima, and see Thorndike, *op. cit.* 5. 438-39. The fact seems to be that Gesner, dying in 1565, lived too early to become acquainted with any but a few of Paracelsus' writings, most of which were published during the subsequent thirty years.

spectacle of a dramatic and masterful personality taking possession of a trend, thrusting it forward, and giving it his name.

As far as England was concerned it was Gesner, not Paracelsus, who was the prime mover of the cause of chemical medicine in its early stages. In addition to his *Euonymus,* just considered, his later book, known as the second part of *Euonymus* and published posthumously in 1569, was translated and edited by the extremely influential English surgeon, George Baker, as *The Newe Iewell of Health* in 1576. Of this we shall have more to say presently. For the moment, it is interesting to dwell upon the point that Paracelsus does not seem to have had any effect on English medicine before the early 1570's.

Thus the important medical works of the 1550's by John Caius[9] and Christopher Langton [10] showed no knowledge of him. Nor did those of William Bullein written in the 1560's.[11] More significant still, the latter decade witnessed the publication of three detailed commentaries on abuses in the art of medicine by Thomas Gale, John Securis, and John Hall, none of which alluded to the Paracelsans, although the occasion would have been a propitious one for doing so. To his translation of Lanfranc's famous work on surgery (1565) John Hall appended *An Historicall Expostulation against the Beastlye Abusers, Bothe of Chyrurgerie, and Physyke, in Oure Tyme* complaining of the quacks and impostors who had entered the medical profession but not associating them with Paracelsism as was usually done in the next three decades when Paracelsus became known in England. The omission is similarly noticeable in John Securis' *A Detection of the Daily Enormities and Abuses Committed in Physick* (1566) and in Thomas Gale's prefaces to his translation of Galen, published in 1586 but written more than twenty years earlier. Gale himself was a surgeon of considerable vigor and independence who would have been likely at least to allude to some of the Paracelsan methods if he had known of them. Yet his *Enchiridion of Chirurgerie* (1563) and his *Antidotarie* (1563), though making a good deal of use of distillations, balsams, and tinctures, seems to derive them from people like Lully and Brunschwig rather than the Paracelsan school. In this respect his *Anti-*

[9] *A Boke Against the Disease Called the Sweate* (1552).

[10] *An Introduction into Phisycke* (1550-?); *A Treatise Declaring the Principal Partes of Phisick* (1547).

[11] *Bullein's Bulwarke of Defence* (1562), *A Comfortable Regiment aginst Pleurisi* (1562); *A Dialogue against the Feuer Pestilence,* (1564)

dotarie forms an instructive contrast with the antidotaries of George Baker (1579) and John Banister (1589), to be discussed later. The lesson to be learned from Gale is that English surgeons were well aware of the uses of some of the more elementary chemical preparations by the middle of the sixteenth century, twenty years before they became aware of Paracelsus.[12]

THE DECADE 1570-1580

As might be expected, the first discoverable mention of Paracelsus in an English medical book was a cry of scorn from one of the established physicians. John Jones was a university man, writer of several medical treatises, and possessor of a fashionable clientele which included two earls. When in his *Diall For All Agues* (1566) he exclaimed against malpracticers and ignorant empirics in medicine he apparently did not know of the Paracelsans. But six years later in *The Benefit of the Auncient Bathes of Buckstones* (1572) he knew them only too well as challengers of all of Galen's theories. Rushing to the defense, Jones gave a prolonged review of Galenism and added contemptuously:

> Let Paracelsus (& all other vayn practisers do what they can) with his fyre Work, of three beginnings of salt, Brimstone, and quicksiluer, blowing the coles unto ashes, and stilling til they haue no glasses, as wyse as geese, as reasonable as apes, as trim as Colyers, and as riche as newe shorne sheepe.[13]

Two years later the title page of Jones' epitome of *Galens Bookes of Elementes* (1574) declared the purpose of "Confuting, as well the errours of âll them that went before time, as that hath, or shal folowe hereafter of the Paracelcians" in explaining the composition of the natural world.[14] This, however, seems to have been an afterthought, since the text itself is not directed expressly against the physical theories of Paracelsus.

Likewise in his final book, *The Arte of Preseruing Bodie and Soule in Al Health* (1579), his reverence for Galen and ridicule of Paracelsus is obvious. On the elements, humors, and spirits of the newborn baby he takes the Galenic attitude:

[12] The numerous English editions of Alessio's *Secretes* beginning in 1558 show a considerable degree of chemical influence but nothing specifically Paracelsan. And Thomas Rainold's *Compendious Declaration of a Certain Lateli Inuented Oile*, written at Venice in 1561, mirrors clearly the rapid advance of the new medicine in Italy even before the onset of Paracelsism. It was quite rabid about the virtues of "faire waters, Baumes, oiles . . . of such forse and efficacie . . . that no outher Simple or compost medicines in times passid diuisid mai be to them in eni wise comparid" (sig. $B4^v$).

[13] To the Reader, fol. ii^r.

[14] See also the dedication to the Earl of Shrewsbury.

Let Paracelsus with hys followers neuer so foolishly fable to the contrarie, of Erastus so excellentlye confuted in Latyn, and by Kinder in Englishe, that I neede not to stay thereupon. . . .[15]

In the margin opposite the name of Erastus is cited "Erastus contra Paracels.," and opposite Kinder "De part. hom." The former reference is clearly to the well known *Disputationum de medicina nova Philippi Paracelsi* by Thomas Erastus printed at Lyons in 1572. This was the most devastating of the many European attacks published against the Paracelsan school and the most often referred to in English medical literature. Such was the scope of its influence that perhaps not without significance is the fact that Jones' first English reference to Paracelsus falls in the same year as its publication. Nothing spreads a doctrine so quickly as a violent criticism of it. The citation to Kinder I am unable to trace.[16] Kinder's book, whatever it was, does not seem to be extant and is not even alluded to by other medical writers of the period.

Now the remarkable fact is that Jones was no enemy of chemistry as such. Several passages in various of his books show his knowledge of the science. For example, in *Bathes of Bathes Ayde* (1572) he wrote of having watched the properties of copper, iron, and marcasite "seuered, and tried" by "the Chimistes and Fyners in London, Cornewall, Cumbreland, and Irelande" in the company of one "John Challoner, a gentleman, not onely excellent in all the Mathematicall Sciences, but also in Chimistrie Phisike and euery part of Phylosophie," and had himself engaged in such experiments "by our owne industrie." [17] What seems chiefly to have roused Jones' derision and ire was Paracelsus' negation of Galen's theories of the composition of matter and the physiology of the human body. It was erroneous theory that made the Paracelsans "vayn practisers" in the eyes of John Jones. This attitude by the first English commentator on Paracelsism was to be prophetic of the future of the movement in England.

For the roots of that movement we must go back to Thomas Hill, who spent his life writing popularizations of several phases of Elizabethan science and died shortly before 1576.[18] An autobiographical statement at the conclusion of his *Profitable Arte of Gardening* (1568) that he "neuer

[15] P. 31. Another slighting reference to Paracelsans is on p. 27.

[16] Jones mentioned Kinder (Kindar) once more, on p. 89, as a writer on comets and blazing stars.

[17] Fol. 20r. See also 18r and 20v.

[18] Francis Johnson, "Thomas Hill: An Elizabethan Huxley," *Huntington Lib. Quart.* 7. (1944) 329-51.

tasted of the learned Laake, but [was] rather alwayes rudely taughte, amonge the Smythes of Vulcanus forge" [19] may mean that he had early training as a metal worker. At any rate, he was definitely interested in medical chemistry. His *Gardeners Labyrinth* (1576), published after his death by Henry Dethicke, displayed woodcuts of furnaces and distilling apparatus for herbs, and his other books used numerous metaphors from the science.[20] Especially his *Pleasant Instruction for the Parfit Ordering of Bees* (1568) described methods of distilling honey into a marvelous quintessence which would dissolve gold, heal desperate wounds, and, administered to "any beyng at the poynte of death, doth so soudainly recouer the parsone again."[21] Since the identical recipe can be found in Fioravanti,[22] whom Hill was translating into English at the time of his death, Hill probably got it from that source.

But Hill's real service to chemical medicine lay not so much in his own books as in his bequeathing of unfinished manuscripts to two men, John Hester and George Baker, who were in strategic positions to spread the new doctrine. To Hester went the manuscript translation of Fioravanti, *A Ioyful Iewell,* published in 1579, which started the long series of publications by Hester more potent than any other single force to make Paracelsism widely known in England. To Baker went Hill's complete translation of Gesner's *Euonymus,* part two, completed and published by Baker as *The Newe Iewell of Health* in 1576. Through Baker the point of view which it represented received a favorable introduction to his friends William Clowes, John Banister, William Goodrus, John Read, and others, composing the most enterprising and enlightened group of surgeons, or, indeed, medical practitioners of any kind, in England at that time. Whether Hill himself was actually a Paracelsan is not certain, and perhaps not very material. Although the name of Paracelsus does not appear in his books, his devotion to Fioravanti, who was a Paracelsan of a sort, perhaps indicates that he was. The essential point, however, is that, through Hester and Baker, Hill made really vital contributions to

[19] Sig. ddiii^{r}.

[20] *A Contemplation of Mysteries* (1571) 20; *Gardeners Labyrinth* (1586) 12, 29, 88; *Contemplation of Mankinde* (1571) fol. 52^{r}. The Preface to this last work shows that Hill studied physiognomy under John Dee.

[21] Fol. 44$^{r\&v}$. At 48^{v} Hill describes "a proffitable Oyle out of waxe" borrowed from Lully.

[22] *A Short Discourse uppon Chirurgerie* (1580) fol. 58^{r}. Baker's edition of the Gesner manuscript he received from Hill, *The Newe Iewell of Health* (1576) fol. 172$^{r\&v}$, has the same recipe which it describes as "borrowed out of the greeke Leonarde Fiorauant." This illustrates well how material passed from Fioravanti to Hill to Baker and Hester.

the development and acceptance of chemical medicine in England.

We may now look a little more closely at Baker's work. In contrast with the uniform hostility of Jones' utterances about Paracelsism in this same decade, Baker's views were mixed. Initially, in the prefaces to his first book, *The Composition of the Pretious Oil called Oleum Magistrale* (1574), he deplored the rashness and fancifulness of the Paracelsan method, that is, the general medical and surgical theory. Both sound reason and broad experience, he emphasized, were necessary to a science of surgery. Galen disapproved of

> the unlearned practisers of his time . . . devising every man a method after his owne way. . . . Such was the method of Thessalus before Galens time, and such is the method of Paracelsus in our time, but what peril did insue of ye rash method of Thessalus: is by Galen in sundry places declared and how great harme hath beene doon and dayly is committed by Paracellistes, both learned Erastus and Desenius with others doo witnesse by their writings and dayly experience it self teacheth.[23]

In spite of Baker's reliance on Erastus, what chiefly troubled him as a common sense surgeon was the vicious practical results of Paracelsan theory. It seemed to him to produce medically illiterate practitioners.

Yet he himself was not averse to recommending a number of specific Paracelsan remedies in completing and publishing Hill's unfinished translation, *The Newe Iewell of Health* (1576). Thus the English text as published introduced prescriptions for "Christes baulme," "An oyle drawne out of Bole Armoniacke," "Oyle of Antimonie," etc., all of which it expressly ascribed to Paracelsus.[24] It also laid Fioravanti heavily under contribution.[25] Since some of the cited works of Fioravanti were not published until after Gesner's death in 1565, these formulae were probably added to Gesner's text by Hill, who was at the same time busy translating Fioravanti in the manuscript later published by Hester. Hill may similarly have added some or all of the Paracelsus prescriptions.[26] Baker was willing to let all these stand in the final version. Taken as a whole, this book was certainly the most advanced and complete work on chemistry and chemical therapeutics published in England in the sixteenth century.

[23] Sigs. C1^{v} & C2^{r}. The oleum magistrale which Baker presents is not Paracelsan in any way despite its suggestive appellation.

[24] Fols. 177^{r}, 215^{v}, 229^{v}, 235^{r}, etc.

[25] Fols. 159^{v}, 172^{v}, 213^{v}, etc.

[26] I have not had access to Gesner's original Latin edition to ascertain whether the Paracelsan allusions were Gesner's or were added in English either by Hill or by Baker.

Baker's preface to it defended the great ancient physicians like Hippocrates, Celsus, and Galen against the attacks of "later wryters," evidently Paracelsus, by insisting that their work formed the foundation of all subsequent development. Nevertheless, Baker went on to declare his own strong faith in the superiority of the new chemical medicines over the old. By "the Chimicall Arte," he said,

> those medicines which are harde and hidden, their forces and vertues are plainly manifested and prooued, and the grosse iuyce being mingled with the subtill and fine substance, are thereby digested and separated, as we may see by the drawing of the oyle of Golde, Iron, Copper, or Tynne. Also by Distillation are corrected the malignitie or venimous qualities thereof, as in oyles of Quicksilver, of oyle of Vitrioll, Antimonie, artificiall saltes, and many other purging medicines. Furthermore, we see plainely before our eyes, that the vertues of medicines by Chimicall distillation, are made more vailable, better, and of more efficacie than those medicines which are in use, and accustomed.[27]

This advocacy of "metalline" remedies is as strong as any pronounced by the outright Paracelsans. Apparently what Baker could not stomach was the abstruse Paracelsan pathology and physiology. But the chemical theory and practice, already familiar to English medicine from many other sources, he could readily assimilate and promulgate. He even took the useful step of recommending several apothecaries from whom the reader might obtain the new preparations:

> one mayster Kemech an Englishe man dwelling in Lothburie, another, mayster Geffray, a French man dwelling in the Crouched friers, men of singular knowledge that waye, another named Iohn Hester dwelling on Powles wharfe, the which is a paynfull traueyler in those matters, as I by proofe haue seene, and used of their medicines to the furtheraunce of my Pacients healthes . . . .[28]

So a connection is established between Baker and Hester, the two legatees of Thomas Hill.

Yet Baker was still believer enough in the old medicine to refurbish an outdated English translation of the medieval surgeon Guy de Chauliac, *Guydos Questions,* in 1579, and also to translate the third book of Galen on the Composition of Medicines, wherein he called Galen "the Father

[27] "George Baker to the Reader." When *The Newe Iewell of Health* was reissued in 1599 under the title *The Practise of the New and Old Phisicke* no significant changes were made in the text.

[28] Conclusion of the Epistle to the Reader.

and light of Phisick."[29] In the same year he published his *Antidotarie,* which offered hundreds of prescriptions "chosen out of al the best Authors that haue written both olde and new." Here indeed was a mingling of authorities from all ages. The moderns were well represented by formulae from Clowes, Vigo, Vesalius, Wecker, Fallopius, and others. Paracelsus appeared only once, as originator of a balm for wounds,[30] but Winter of Andernach, whose *On Medicine Old and New* (1571) had given generous recognition to Paracelsus, supplied dozens of formulae.[31] Conceivably Baker learned the single Paracelsan recipe from Andernach.

In later years Baker contributed prefatory essays to divers books by his friends Clowes, Banister, Gerard, and Lowe but wrote nothing further to show the development of his views on the new medicine.[32] From what we have seen of his work and that of John Jones it is clear that Paracelsism was not an especially powerful force during this first decade of its introduction into England. Allusions to it were scattered and fragmentary. Its general medical theory, known largely through the refutations by Erastus, Desenius, and Kinder, was rejected by both Jones and Baker. The latter, already a warm partisan of chemistry in medicine, looked with favor on particular Paracelsan remedies because they shared this method. At least three apothecaries, Kemech (or Keymis), Geffray, and Hester, were selling chemical medicines. But it looks very much as if Paracelsus rode into England on the shoulders of medical chemistry, instead of carrying it there himself. We are now to see how, by the vigor and notoriety of his character, he began to appropriate the new movement.

The Decade 1580-1590

In the opening years of this decade Paracelsism found in John Hester a spokesman who was thereafter to lead in the work of publicizing it. From his apothecary shop in Paul's Wharf he widely advertised and sold the prescriptions of Paracelsus. And, even more influentially, he translated books by Paracelsus and his followers Fioravanti and Du Chesne

[29] Fol. 54^{v}.

[30] Fol. 100^{r} of the copy bound in with Guydo.

[31] See the sections in the *Antidotarie* on baths, cataplasms, plasters, etc. Winter of Andernach is discussed in Thorndike, *op. cit.* 6. 219.

[32] With the possible exception of Baker's new edition of Vigo in 1585, which I have not seen. Baker contributed essays or verses to Clowes' *De morbo gallico* (1585), Gerard's *Herball* (1597), and Lowe's *Whole Course of Chirurgerie* (1597), besides the earlier *Treatise of Chyrurgerie* (1575) by Banister.

which had wide currency during both the sixteenth and the seventeenth centuries.

Hester first appears as the man to whom the dying Thomas Hill handed his unfinished translation from Fioravanti, *A Ioyfull Iewell* (1579), requesting him to set it forth in print. Becoming interested in the Italian physician, Hester then undertook on his own account translations of two of Fioravanti's other books, *A Short Discours uppon Chirurgerie* (1580) and *A Compendium of rationall Secretes* (1582). In fact, Hester became so enthusiastic as to plan translations of all of Fioravanti's other treatises,[33] but apparently gave over the project at some time after 1582. The superficiality and arrogance of the Italian might well have cooled his ardor for the task. Fioravanti was indeed little more than an exalted kind of quack possessing a few nostrums to which he gave grandiose titles like Magno licore, Electuary angellica, and Pillole aquilone, any one of which would presently revive the patient from his very deathbed. The *Chirurgerie* was a highly empirical work, ignorant of anatomy, indifferent to an understanding of the causes of disease. Likewise the *Iewell* and the *Compendium* gave the impression that all the doctor needed to know was how to distill a few drugs in some unusual combinations.

Small wonder that orthodox physicians who had labored to master the complex theories of Galen were horrified at the unabashed empiricism of many of the new practitioners and inevitably coupled them with the vagabonds, mountebanks, and ruffians who roamed the English countryside under the guise of doctors to extort money from the sick. For Hester, however, who had not been trained in Galenic medicine, Fioravanti had definite points of appeal. Here was a doctor who proclaimed new remedies, some of them metallic, cheaper to prepare than the old, easier to preserve, promising much speedier cures, affording hope in cases heretofore considered incurable. Like Paracelsus, Fioravanti was definitely conscious that he was a member of a new, growing school of medicine opposed to and better than the old.[34] He had the same kind of brash, dramatic egoism. He stressed practice as against mere theory in much the same way. He favored letting nature be the chief healer of her own

[33] Hester's Address to the Reader in Fioravanti's *Short Discours uppon Chirurgerie*, Sig. A3^{v}.

[34] Read especially Fioravanti's preface to his *Chirurgerie*, fol. iii^{v}.

wounds, as did Paracelsus.[35] We cannot be sure that Hester in the early 1580's when he was translating Fioravanti, was explicitly a Paracelsan, but we can feel certain that the type of medical program which he discovered in Fioravanti attracted him to the translation of Paracelsus himself and of one of his main apostles, Du Chesne. As will become clear, Hester was primarily a craftsman in pharmacy and cared but little for the remoter reaches of Paracelsan medical theory.

Hester's prefaces to Fioravanti, however, display an awareness that the material translated calls for some defense. In dedicating the *Compendium* to Richard Garth, for example, he argued strenuously that science cannot remain static but must change and advance. Each generation must rely on its own findings and not on those of its predecessors. It is true, he admits, that the new chemistry is being abused by ignoramuses, but "this is not the fault of the matter, but of the men." He urges his readers to be wary how they use it. The whole tone of Hester's prefaces presupposes a fierce opposition from the more orthodox in the medical profession.

Although Hester produced two more translations in the 1580's, he added nothing of consequence until he published his versions of Paracelsus and Du Chesne in 1590. Discussion of these may be deferred. He continued, nevertheless, to print large bills advertising the chemical preparations for sale in his shop. One of these, beginning *These Oiles, Waters, Extractions, or Essences . . . to be Solde, by Iohn Hester . . .* (1585?) includes the famous stiptic plaster of Paracelsus and offers "for a reasonable stipend, to instruct any that are desirous to learne the secrets of the same in a few dayes. . . ." How quickly Paracelsan medicine might be spread by such teaching may readily be imagined. These activities, together with his translations, made Hester the most potent carrier of Paracelsan doctrine at this time in England, but always on a very practical level.

The cudgels were taken up for Paracelsan philosophy on the theoretical plane by one R. B., whose book, *The Difference betwene the Auncient Phisicke and the Latter Phisicke* (1585), gave a long and impassioned exposition and defense of the system as it related to medicine. The identity of R. B. is matter for speculation, but the religious cast of his argument makes him sound very much like a clergyman. His treatise begins with a

[35] *Chirurgerie*, fol. 7v. Cf. Paracelsus, *Chirurgia magna*, Pars prima, Tractatus primus, Caput II: the natural balsam of the body heals wounds. The surgeon can only assist it.

24-page "Obtestation to almightie God" accusing Galenism of being atheistic. The main point, developed with a rather shrill fervor, is that Galenism tends to give materialistic explanations of human personality and to impugn the separate spirituality of the soul.[36] Paracelsism, on the other hand, teaches that God's power works immanently in the human body and soul as in all other things. R. B.'s religious emphasis was a retaliation to the charges of heresy aimed at Paracelsus by Erastus and Aubertus.[37] He proceeded to expound the Paracelsan pathology, declaring that disease is caused by impure seeds taken into the body with the food it eats.[38] As long as the person is at unity with himself these seeds are expelled naturally, but when there is a breach of internal concord they must be ejected by medicines which are of two kinds, one Universalis or ethereall, the other Ternarii or particular and composed of the three underlying substances, Salt, Sulphur, and Mercury.[39] Though each disease is an excess of one of these three substances in the body, it must be amended by administering the same substance which is in excess, not a contrary one.[40] The Galenists err in administering contraries and in believing that disease results from an unbalance of the four humors. These are merely incidental. All medicines are to be prepared by fire, which separates their gross from their pure constituents.

R. B. goes on to summarize the history of chemical medicine from Adam down.[41] He insists that it is the truly ancient science, whereas Galenism is but a recent upstart. Although Paracelsus was not the inventor of this genuine science he has restored it to its pristine purity. The remainder of R. B. 's space then is given to a refutation of the slanders of Erastus upon Paracelsus' religious probity.[42] At every point, Paracelsus can be proved to have been quite in accord with the Bible and holy doctrine. In the course of this plea, R. B. manifests an acquaintance with a number of the principal works of Paracelsus, including his *Labyrinth, Paramirum,* and *Address to the Athenians.*

R. B. 's work is worth dwelling on because it was the only full-

[36] Sig. A5^{v} ff.

[37] Erastus, *De medicina nova* (Lyons 1572) 1-29, and the Epilogus, 234. Jacobus Aubertus, *Iacobi Auberti Vindonis de metallorum ortu & causis contra chemistas breuis & dilucida explicatio* (Lyons 1575) 8. Aubertus relies mainly on Erastus.

[38] Sig. B4^{v} ff.

[39] Sig. B1^{v}, B6^{v}, C3^{r} ff.

[40] Sig. C3^{v}.

[41] Chap. 10, sig. F4^{r} ff.

[42] Chap. 20, sig I3^{v} ff.

panoplied defense of Paracelsan medical philosophy in English in the sixteenth century. Yet it does not seem to have made any great stir in English thought. Nobody answered it or even mentioned it, as far as I can discover. The best explanation of its lapse into oblivion is that Englishmen did not particularly care what was the Paracelsan theory of disease. Though they were ready to yield ground in the Galenic pharmacy, Galenic humoral pathology was good enough for them.

So the same neglect befell a smaller companion-piece to R. B. 's book, a publication in 1586 by I. W. called *The Copie of a Letter Sent by a Learned Physician to his Friend, Wherein are Detected the Manifold Errors used Hitherto of the Apothecaries.* This likewise rejected the Galenic theory that a disease was an excess of one of the four humors in favor of the Paracelsan view that it proceeded from poisons within food. All substances contained both poison and nutriment.[43] Treatment in the furnace would separate out the poisons and render beneficial even inorganic things like opium, mercury, and arsenic, usually considered most venomous. Galenic vegetable compounds, on the contrary, merely added poison to poison. I. W. also had much to say in support of the personal morals and religion of Paracelsus who labored so diligently in this science that, although it began from Adam, "it seemeth to be borne a new with him."[44]

We may wonder why the personal morality or immorality, piety or impiety, of Paracelsus should be so keenly at issue in a debate about the merits of his work. But for the Renaissance the character of the leader of a movement helped to determine the judgment to be formed of the movement itself, and in some measure the same is still true today. Erastus had vehemently raised the religious and moral issue in challenging Paracelsus. The defense quite naturally retorted in kind. It is interesting to notice that these defenders, while acknowledging that chemical medicine had had a long previous history, essentially looked to Paracelsus as their real leader. In their eyes chemical medicine was Paracelsism. For them his fame had taken over the new science as if it were a property of his. But most English doctors, even those favorably disposed towards chemical medicine, were still quick to distinguish it from Paracelsan theorizing.

At this same time in the middle 1580's, when R. B. and I. W. were

[43] Sig. B6[r] ff. [44] Sig. A1[v].

publishing their apologies for Paracelsus, the able surgeon John Banister, a member of the Clowes-Baker-Goodrus circle, was starting to take cognizance of him also. Banister's works published in the previous decade, the *Treatise of Chirurgerie* (1575) and *The Historie of Man* (1578), had ignored him. The former had been drawn out of Calmateus and Johannes Tagaltius. Although its various prefaces written by Banister himself, Clowes, Baker, Richard Smith, and Thomas Newton complained angrily of the charlatans swarming in medical practice, there is no real reason to think that they had Paracelsans in mind. *The Historie of Man* was an anatomical treatise which accepted the new findings of Vesalius and Columbus against the authority of Galen but went out of its way to object to the defaming of Galen.[45] Paracelsus was never mentioned. By the 1580's, however, his name began to turn up frequently in Banister's books, and always in a favorable context. In 1585 Banister collected *A Compendious Chirurgerie* out of Johann Wecker, who himself several times cited Paracelsus favorably as an authority on aneurism.[46] To each chapter of Wecker Banister appended annotations of his own revealing great admiration for Paracelsus' works on surgery, the *Chirurgia magna* and *Chirurgia minor*.[47] Unguents, plasters, and powders by Paracelsus were recommended, and at one place a whole page was devoted to his method of dressing wounds.[48] Moreover, recipes for wound drinks and gunpowder burns were adopted from Du Chesne (Quercetanus), the French Paracelsan scholar.[49]

Similarly, Banister's *Antidotarie* (1589) is a vast advance over the prior *Antidotarie* (1579) of George Baker, on which it was clearly based. A comparison of the two books indicates that Banister copied Baker's alphabetical classification of medicines, as well as many of the individual prescriptions under each heading. His main contribution, however, was to add a large number of new prescriptions culled from English contemporaries, and notably from continental Paracelsans. Whereas Baker had but one formula from Paracelsus, Banister listed several dozen in the sections on wound drinks, plasters, and unguents.[50] Besides, Banister added decoctions and ungents from Du Chesne,[51] purgations from Fioravanti [52] (probably derived through Hester's translations), and liniments

[45] Eg. sigs. B2^{r}&^{v}, B4^{v}, B5^{r}.
[46] Eg. pp. 142, 161.
[47] Eg. pp. 164-6, 178, 182-83, 213, 243.
[48] P. 184.
[49] Eg. pp. 222, 225, 244.
[50] Pp. 97 ff., 265, 296-97.
[51] Eg. pp. 100-101, 288, 305.
[52] Eg. p. 229, 233.

from Leonhard Thurneysser,[53] the Swiss Paracelsan, none of whom had any place in Baker. The net result was that the Paracelsans were almost as well represented as the ancients. Authorities of the two schools were interspersed with one another quite impartially, often on the same page. With a fine eclecticism, Banister took whatever seemed best from both.

The natural consequence was that he did not altogether please either one. We gather from some rather cryptic comments in Clowes' preface that Banister was under fire from the conservatives. Clowes himself had plenty of experience with the same blind resistance to change. He and Banister were moderates who had to contend with opposition from extremists on both sides. And both men had reached in common with their friend George Baker a strong decision to read the Paracelsans for definite medicines and limited methods useful in surgical practice rather than for the larger theory.

Clowes was, indeed, the man in whom the struggle between the new and the old is most vividly realized during the last two decades of the sixteenth century. By temperament a fighter, he filled his books with tirades against his enemies of every complexion. Short-tempered, sensitive, gifted with a scorching literary style, forward looking in his surgical ideas, centered in a group well abreast of the new movement in chemical medicine, he found much to resent both among the ultra conservatives and among the shallow practicers who called themselves Paracelsans.

His encounters with the latter seem to have begun early in the 1570's. The first edition of his *Treatise touching the Cure of Morbus Gallicus,* printed before 1578,[54] is not extant, but subsequent editions show that in it he set out to expose and discredit "one Valentine Rarsworme of Smalcalde," a foreign surgeon who in 1574 had a reputation as "a Wyse Alchimist" and apparently a sizeable body of followers in London.[55] Clowes itemized the names of patients whom this charlatan had killed by his malpractice. One of his disciples then published an attack on Clowes' book, especially on the two points of his use of unquenched quicksilver for syphilis and of incarnatives for wounds.[56] No copy of this

[53] P. 177, 281, 289-90.

[54] The *Short Title Catalogue* lists the 1579 edition as the first. But Clowes' own commendatory verses to Banister's *Historie of Man* (1578) state definitely that his *Morbus Gallicus* had already met with an unfavorable reception in some circles.

[55] The 1585 edition, fols. 9[r] ff. Clowes seems to have made allusion to this quarrel as early as 1574 in some Latin verses appended to Baker's *Oleum magistrale* (1574), speaking of the frauds who came to England from Smalkalde.

[56] Fols. 24[r] & 31[v] of Clowes' 1585 edition.

attack seems to have survived, nor is the author's name specified, but from Clowes' replies in the 1585 edition of *Morbus Gallicus* it seems likely that the antagonist was quoting Paracelsan doctrine against Clowes. The latter refers to his opponent in these terms:

> But as for his owne presuming knowledge and iudgment in Alchimie, it was yet neuer knowne worth the speaking of: but if he will needes be one, I thinke I may truely iudge him to be one of the number of them and the formost of the rancke, which Maister Iones wryteth of in his booke of *Buckstones Bathes*: Which bloweth the coales unto asshes, and stilleth till they haue no glasse: as wise as Geese, as reasonable as Apes, as trimme as Colliers, and as riche as newe shorne Sheepe.[57]

We have seen earlier that this passage in Jones was a sneer at the Paracelsans. That Clowes had them in mind here is proved also by the fact that he introduced the whole Rarsworme episode with an affirmation that Galen and Hippocrates were still "the two golden fountaines and well sprynges of Phisicke and Chirurgerie . . . although some of late . . . hath dispightfully heaped up against them, a hewge laystale or dounghill of untrothes." [58] The allusion to Paracelsism was made explicit by an essay of William Pickering's appended to Clowes' book, indicting

> one Valentine Rarsworme, a man singularly experimented in deceits, voyd of knowledge, a deceitfull Alchumist, a proud pratling Paracelcian, & yet in deed no Paracelcian, but a usurper of that name. I speake not against the good workes of the right Paracelcian, no more doth the author, with whome I haue conferred with in Alcumie, but those Empirickes who indeede are schollers of Thessalus, such as are and haue bene whipped and banished from Citie to Citie, such was his maister Rarsworme, whose desciple he was.[59]

According to Pickering, then, both he and Clowes were respecters of chemistry in medicine, but they could not abide its abuse by quacks and mountebanks. The battle against untrained empirics in England had been a hard one, and was still going on. Parcelsism unfortunately seemed to be a natural ally of quackery. Its simplification of the Galenic pharmacopeia, its contention that it could effect short easy cures with metallic medicines cheaper to get and simpler to transport than the complex syrups and electuaries of old, made it only too readily adaptable to the purposes of the cheating empiric as he traveled from town to town with

[57] Fol 12v.
[58] Fol. 8v.
[59] Fol. 59r.

his box of drugs. This close association of Parcelsism with charlatanry provided a powerful weapon for the conservatives who wanted nothing to do with it under any circumstances. And those doctors who thought there might be something in it were obliged to make the distinction between false and genuine Paracelsism which Pickering labored to establish.

Highly significant in this regard was Clowes' epistle to his next book, *A Prooued Practise for All Young Chirurgians* (1588). There he first struck at the old guard in the profession who did not want to see any medical books published at all. Then he turned upon "another sort of domesticall enemies" who

> do most idiotlike behaue themselues against Hypocrates and Galen, and all Galenists . . . .And some of them are scarce able to write and reade true English, but doe brag without measure (upon a very sudden) of their excellent cunning, which happely they had stolne out of the beggerly budgets, of some counterfeite stale Paracelsian Quacksaluer, or Mountibancke, and yet moreouer do challenge, to be the true Disciples of Peracelsus, but God knowes, they are not worthy to serue under his regiment for pyonors: for they missinterprete Paracelsus whome truely they understand not, and condemne Hipocrates and Galen whome they neuer read. I would no man should thinke go heere abouts to speake against the thing that is good . . . for truth is truth from whome so euer it come, be it from Hipocrates, Galen, Paracelsus, or any other learned men.[60]

No attitude could be fairer or more sensible. Clowes says he is not a fanatic of either party. Yet one has the feeling as he reads on into the book that whenever Clowes actually met practicing Paracelsans they always turned out somehow to be of the false breed. Reverting scathingly once more to his former antagonist who had criticized him in the matter of the quicksilver and the incarnatives, Clowes ridiculed him for his "exalted essences foolosophically extracted" and his "Spirits that inhabit or dwell in the ayre, called Nenufareni, and Aerdadi."[61] These latter were lesser gods in the Paracelsan philosophy already censured by Erastus.[62] Clowes also lashed him for rejecting Galen on the ground that he was heathen. Warming to his work, Clowes then alluded sarcastically to a poisonous pill administered to patients by one of the opposing party.[63]

[60] Fol. 4r&v of Epistle to the Reader.

[61] Sig. A1v. See Paracelsus, *Philosophiae ad Athenienses Liber I*, Textus XIII: "Neufareni in aere Terrae, seu in poris terrae habitant." The Aerdadi were described by Paracelsus in *De vita longa*, cap. III.

[62] *Op. cit.* 17.

[63] Sig. A2r.

Later in the book he detailed in vigorous narrative his contest with a traveling surgeon from overseas, gaudily garbed, who used Paracelsan methods in healing wounds and claimed miraculous powers for the only three medicines he bothered to carry around with him—a quintessence which would keep a man alive until Judgment Day, an oil, and a plaster scarcely less unique.[64] Nevertheless, on other pages Clowes thought it not amiss to approve several plasters and unguents of Du Chesne and Paracelsus.[65] In particular, he spoke of the Paracelsan plasters "practised of late in the Lowe Countries by a worshipfull Gentleman called maister Ierom Farmer" and given to Clowes by him "at that time when Nemegam was besieged." [66] That was when Clowes accompanied Leicester's expedition to the Low Countries in 1585.

This passage, with others like it, suggests strongly that Paracelsism was not flowing into England by the route of books alone. English surgeons were picking it up in their travels abroad. And Valentine Rarsworme and his ilk were bringing it into the country continually in degenerate form. There was in fact too great a traffic of foreign physicians through England to suit the native practitioners, who protested anxiously against their inroads. The writings of Thomas Gale, Franciscus Arceus, Baker, and Clowes are eloquent on this subject. Listen to Thomas Gale, writing in his translation of Galen (1586):

> I will not speake of a multitude of strangers, as pouch makers, and pedlers, with glass makers, and coblers, which runne out of their owne countries, and here become noble Phisitions and Chirurgions, such as now is most in estimation, and ruleth all the roast in our Countrie, so that the poore English men, and such as haue serued in the time of warres . . . when they come home againe, they, their wiues, and their children, may goe picke museles, for this iollie companie afore spoken off, haue taken up all their acquaintance whilest they be seruing in the warres.[67]

Inasmuch as Paracelsan teaching was becoming widely known on the continent as early as the 1560's, it seems indubitable that physicians and

[64] Sig. P6v.

[65] E.g. pp. 5, 61, 81, 87, 89.

[66] Pp. 52-53.

[67] P. 37. See also Baker, *Oleum magistrale*, fols. 43v-45r. Arcaeus, *Compendious Method of Curing Wounds* (1588) "To the Reader," describes foreign quacks at work in England, especially "a flemming . . . named Woolfgange Frolicke" and "one Peter Ballet, a Dutch man borne." Since on the next page Arcaeus makes a contemptuous reference to Paracelsus, it seems probable that he identified these impostors as Paracelsans. Clowes' *Prooued Practise*, p. 1, denounced "Iosephus Petrus, alias Scleeues" as one of the "Fugitiues and Landlepers, which runne and gad from Countrey to Countrey. . . ." There was an immense amount of complaint of this kind coming from English medical men. Note that Paracelsism was especially strong among the Dutch and Germans.

demi-physicians coming into England from abroad served as one of the chief means of transmission. This foreign provenience cannot have done much to render it palatable to English medical men either.

Under these circumstances the ease with which Paracelsism made headway in England in this decade of the 1580's is rather astonishing. It found determined propagandists in Hester, R. B., and I. W., and a more reserved and limited but still helpful admirer in John Banister. Also, it never aroused a frontal counter-attack like those of Erastus, Aubertus, and Fenotus on the continent. Its English opponents seem to have been content to rely on the European broadsides and to do a good deal of oral sniping.[68] Clowes and Arcaeus represented the middle-of-the-roaders who were willing to concede some virtue to the doctrine but, angered by its misuse, leaned rather back towards Galenism. Few English doctors were interested in a complete break with the past. They were feeling their way tentatively towards a wider employment of chemical therapeutics. On the whole, it was a highly fortunate thing for Paracelsism that not the outright Galenists but a relatively liberal group of surgeons were publishing most of the books in this crucial decade of its infiltration.[69]

The Decade 1590-1600

By the opening of the 1590's Hester deemed the time ripe for publishing an English translation of a work by Paracelsus himself, *An Excellent Treatise Teaching howe to Cure the French Pockes* (1590).[70] Significantly enough, he chose to dedicate it to "the Maister Wardens, and generall Assistants of the fraternitie of Chirurgions in London," saying that when he considered who would receive matter "harshe to the hearing of many, as it is in the feeling of others odious, I founde none likelier to giue countenaunce thereunto, then you my Maisters of Chirurgerie."[71] Certainly there could be no stronger proof that the administrators of the

[68] R. B., *op. cit.* sig. D1r&v, gave a most interesting account of a debate he had with a Galenist physician at a Bishop's dinner table.

[69] John Ewich's *Of the Duetie of a Faithfull and Wise Magistrate* (1583) contained several slurs at the Paracelsans as vainglorious braggarts who kill many a man with their medicines (e.g. pp. 15r, 16v). Timothy Bright's *Treatise of the Sufficiencie of English Medicines* (1580) pp. 42-43, 50, gave strong support to chemical medicines, without mentioning Paracelsus. I have not seen Bright's three Latin works on medicine, published 1582 to 1584.

[70] As a matter of fact, this was really a translation of a Dutch rendering by P. Hermanni of two of Paracelsus' works; *Von frantzösischen Kranckheit* and *Vom Holtz Guiaco gründlicher Heylung.*

[71] Sig. iir.

Barber-Surgeons' Company were hospitable to Paracelsan doctrine. And Hester's translation of Du Chesne's book on gunshot wounds in the same year, *The Sclopotarie Of Iosephus Quercetanus* (1590), although officially dedicated to the Earl of Essex, made admiring mention of the writings of surgeons Thomas Gale and William Clowes.[72] Evidently Hester looked for his chief support to the English surgical profession, as distinguished from the Physicians, who were separately incorporated in The Royal College of Physicians. Between these two unhappily sundered branches of the medical profession there was, in fact, tense rivalry, the physicians attempting to maintain jurisdiction over the surgeons and to exclude them from administering all internal remedies to the sick. The surgeons were much the more progressive party in Hester's day. Under the leadership of Gale, Hall, Baker, Banister, and Clowes, they were absorbing the new anatomical discoveries of Vesalius, the surgical techniques of Paré, and, as we have witnessed, the pharmaceutical chemistry of Gesner and Paracelsus. Best of all, they were foremost in the drive to make this knowledge available to the common reader in English. Sharply in contrast is the rigidity of the physicians, who were not generally receptive to the new ideas and not eager to publish what they knew. Among them undoubtedly stood the chief adversaries of Paracelsism. Although the record of their opposition is mainly inferential, we may surmise with some assurance that what they called prosecutions of quackery, diligently pressed all during this period, were not seldom really prosecutions of Paracelsism. Accounts of these legal hearings are vividly given in Charles Goodall's book, *The Royal College of Physicians of London.*[73]

As in his books of the previous decade, Hester's prefaces of the 1590's continued to be briefs arguing the case of the new medicine. He denounced the servile compilation of treatises out of old authorities, especially in the instance of a disease like French pox, which was unknown to the ancients.[74] He pleaded that science must go forward or die.[75] He stressed, like Paracelsus, the need for more experience and factual observation in medicine as against unfounded speculation.[76] He expressed doubt that the transmutation of other metals into gold was possible and urged that

[72] In the Epistle to the Reader.
[73] (London 1684) 306 ff.
[74] *Treatise on French Pockes,* fol. iiv.
[75] *Sclopotarie of Iosephus Quercetanus,* Epistle to the Reader.
[76] *Aunswere of Iosephus Quercetanus,* Hester's address to the reader.

energies should be expended, instead, on the more useful business of bringing to light new remedies. His own predilections were revealed in his statement in the Raleigh dedication that he translated the new writers "not for their methode, which I meddle not with, but for their medicines, which I usually make. . . ." Here again is the familiar antipathy to abstract theory.

The contents of the Paracelsus and Du Chesne works which Hester translated, as well as his more independent book, *The Pearle of Practise* (1594), were congruous with this point of view. It was Paracelsism as an apothecary saw it, a repertory of oils, tinctures, quintessences, pills, and waters, frequently of the homely, trifling kind designed to meet the small accidents and occasional needs of the Elizabethan household. Hester did, however, make one or two excursions into Paracelsan theory. He translated in 1591 Du Chesne's *A Breefe Aunswere of Iosephus Quercetanus to the Exposition of Iacobus Aubertus Vindonis, concerning the Original and Causes of Metalles.* Aubert's book published at Lyons in 1575 had been one of the more damaging Galenic polemics against Paracelsus, denying his theory of the subterranean formation of metals and repeating some of the personal slanders first broached by Erastus. Du Chesne countered with a further discussion of the origins of metals and a defense of their employment in medicine. Galen and Hippocrates, he argued, were excellent physicians, but they did not know everything.[77] Paracelsus may have been a heretic in religion, but he resuscitated and promoted the best traditions of chemical medicine practiced by a long line of admirable predecessors.[78] The moderation of Du Chesne's tone compares favorably with the hostile vehemence of that other apology for Paracelsan theory in English by R. B.

Such theory at second hand was likewise voiced in Hester's translation of *Centum quindecim curationes experimentaque,* falsely attributed to Paracelsus and first published abroad in 1582. Hester here was deceived by one of the many works circulating wrongfully under the name of Paracelsus. It was prefaced with introductory essays by Penotus and Johann Isaac Hollandus asserting that "the cure of most dangerous diseases lieth hidden in mettalles" and expounding the Paracelsan classification of all disease under the four types of leprosy, dropsy, gout, and

[77] Fol. 6r.

[78] Fol. 1r&v.

epilepsy.[79] Hollandus also discussed salt, sulphur, and mercury as the three truly elementary principles.[80]

Hester died between 1591 and 1594, but his influence continued to grow. We have seen that several of his books appeared posthumously soon afterwards. Even in the seventeenth century, moreover, his translation of Fioravanti's *Chyrurgery* was given another edition (1626),[81] and his Paracelsan treatise, *The First Part of the Key of Philosophy* (1596), was reissued as *The Secrets of Physick and Philosophy* (1633). Possibly Thomas Timme's translation of Du Chesne, *The Practise of Chymicall, and Hermeticall Physicke* (1605) was directly inspired by Hester's pioneering attention to that author. Timme became one of the principal interpreters of Paracelsan philolophy in the early seventeenth century, as his *Dialogue Philosophicall* (1612) confirms. At all events, Hester's claim as the high priest of Paracelsism in England during the closing decades of the sixteenth century can hardly be disputed. He joined the movement early, found it in its infancy, and left it mature. He published more of its documents than any other Englishman and, no doubt, from the strategic vantage of his apothecary shop issued more of its medicines. Gabriel Harvey wrote well when he called him "the great Alchymist of London."[82]

One excellent indication of the change of climate in the 1590's from the 1580's is the mellowing William Clowes underwent. His 1596 edition of *De morbo gallico* omitted all reference to the affair Rarsworme, together with the manifold unpleasantness connected therewith. Clowes also added in defense of his use of quicksilver "a note deliuered unto me by a friend, of the nature and qualitie of quicksiluer, according to the minde of Paracelsus, in his booke called Congere. . . ."[83] Since this was only one of many instances in which Clowes got Paracelsan material from his friends and acquaintances, one wonders whether he found first-hand reading of the Swiss master very congenial. In his *Profitable and Necessarie Booke of Obseruations* (1596), which was largely a re-

[79] Sigs. A4^{v} and F1^{r}.

[80] P. 30ff.

[81] A book published in London in 1652, *Three Exact Pieces of L. Phioraunt,* including also the pseudo-Paracelsus' *A Hundred and foureteene Experiments,* sounds like a compilation and reissue of Hester's translations, but I have not had opportunity to examine it.

[82] See the marginalia on Harvey's copy of Hester's *These Oiles, Waters, Extractions,* 1585 (?), now in the British Museum.

[83] Chap. 7, p. 169. Clowes' reference to "Congere" is to *Congeries Paracelsicae chemiae de transmutationibus metallorum,* (Frankfurt 1581), a selection from the works of Paracelsus made by Gerard Dorn.

casting of his *Prooued Practise* with some amplifications and omissions, he continued to warn that "new medicines make not a new art," [84] but commended a plaster from Paracelsus and several times praised Du Chesne as a man "liuing in the light of all good learning."[85] If Clowes ever felt any resentment towards Hester it had disappeared by this time, for Clowes advised his readers to look into Hester's translation of Du Chesne's *Sclopotarie*.[86]

Shortly before his death in 1604, he arrived at the position of adopting the notorious Paracelsan laudanum in his final book, *A Treatise for the Artificial Cure of Struma* (1602). This formula, he says, "I obtained of a very deere friend." [87] But it is in the Epistle to the Reader that he uttered what was to be the final wisdom not only for himself but also for his countrymen:

> I had heere likewise thought good to haue spoken somewhat of Paracelsus, but I must confesse his Doctrine hath a more pregnant sense, then my wit or reach is able to construe: onely this I can say by experience, that I haue practised certaine of his inuentions Chirurgicall, the which I haue found to be singular good, & worthy of great commendations. How be it, much strife I know there is betwen the Galenistes and the Paracelsians. . . . Notwithstanding, for my part I will heere set up my rest & contention. . . . That is to say, if I finde (eyther by reason or experience) any thing that may be to the good of the Patients, and better increase of my knowledge & skill in the Arte of Chirurgery, be it eyther in Galen or Paracelsus; yea, Turke, Iewe, or any other Infidell: I will not refuse it, but be thankeful to God for the same.[88]

This brushing aside of the religious issue as irrelevant to a scientific topic, this honest confession of inability to understand the Paracelsan metaphysic, coupled with a fair-minded selection of whatever had genuine utility in its medical techniques and pharmacopeia, epitomizes the history of the movement in England.

During the decade of the 1590's almost every well-informed person in England came to know at least something about Paracelsism. The references to it both in medical and in non-medical literature are legion. William Clever's *The Flower of Phisicke* (1590) very frequently cited Paracelsus' views on the temperaments, sometimes favorably, sometimes unfavorably.[89] Sir Hugh Platt became an ardent devotee of chemical

[84] P. 27.
[85] Pp. 17, 26, 51.
[86] P. 51.
[87] P. 46. Paracelsus had described this laudanum in his *Chirurgia magna*. It had been subjected to detailed criticism by Aubertus, *op. cit.* 9 ff.
[88] Sig. A2v.
[89] Eg. pp. 25, 48, 73, 76, 78, 81, 93-94, etc.

medicine and seems to have read widely in Paracelsus, especially the *Archidoxa* and the *Labyrinth*.[90] His book, *The Iewell House of Art and Nature* (1594), spoke warmly of many Paracelsan medicaments, while deploring the carelessness with which they were sometimes prepared.[91] He held acquaintance with some of the London chemists, Keymis, Gowthrowse, and possibly Hester.[92] A late increment to the Clowes-Baker-Banister circle was the Scottish surgeon Peter Lowe, whose *Whole Course of Chirurgerie* (1597) made use of Paracelsus' *Magna chirurgia* on wounds.[93] To this volume verses were contributed by John Norden, another recruit to Paracelsism as his cosmic poem, *A Store-house of Varieties* (1601), makes very plain.[94] Also connected with Clowes, Baker, *et al.* was the famous surgeon and botanist John Gerard, author of *The Herball* of 1597.[95] Gerard himself expressed no overt loyalty to Paracelsus, but one of the prefatory addresses in *The Herball* was written by Stephen Bredwell, who made the far-sighted proposal that a chemistry lecture be endowed for the proper teaching of the science:

> If (neuerthelesse) unto that Phisicke lecture lately so well erected, men who haue this worldes goods shall haue harts also of that spirit, to adde some ingenious labourer in the skill of simples, they shall mightily augment and adorne the whole science of Phisicke. But if to that likewise they ioyne a third, namely the art of Chimicall preparation; that out of those good creatures, which God hath giuen man for his health, pure substances may be procured for those that be sicke . . . this present generation would purchase more to the perfection of Phisicke, than all the generations past since Galens time haue done: that I say, nothing of this one fruit that would growe thereof, to wit, the discouering and abolishing of these pernicious impostures and sophistications, which mountpromising Paracelsians euery where obtrude, through want of a true and constant light amongst us to discerne them by.[96]

We observe with interest that Bredwell, a Galenist physician, while condemning the Paracelsans by name, really adopts the substance of their program. Herein lies one of the frequent ironies of the reform movement.

[90] See Platt's *New Fire of Cole-balles* (1603) sig. D3v; *Sundrie Remedies against Famine* (1596) sig. B1r; *Art of Setting Corn* (1601) sig. C2v. At his death Platt left many unpublished manuscripts, now in the British Museum, on chemical therapy.

[91] Sig. B3vff. Bound in with the 1594 edition was *Diuers Chimicall Conclusions concerning the Art of Distillation*, also by Platt, dealing with the extraction of vegetable oils.

[92] Keymis was mentioned in *Chemicall Conclusions*, p. 9; Gowthrowse (perhaps Goodrus?) in *Iewell House*, p. 9; and Hester may be mentioned in *Iewell House*, p. 35.

[93] E.g. sig. R3r.

[94] Sig. F1v of Norden's book.

[95] To Clowes' *Morbus Gallicus* (1585) were appended commendatory verses from "Iohn Gerard Chirurgion to his friend Maister William Clowes."

[96] Sig. B4r.

In the more popular literature the echoes of the Paracelsan controversy were likewise rife. Henry Chettle's *Kind-Hartes Dreame* (1592) contained a violent diatribe against the poisonousness of metallic medicines.[97] The inherent conservatism of the satirist, Thomas Nashe, was summed up in his allusion to "a mettle-bruing Paracelsian, hauing not past one or two Probatums for all diseases." [98] Shakespeare in *All's Well That Ends Well* more tolerantly placed the two sects of physicians on equal terms without commenting directly on either side.[99] *The Differences of the Ages of Mans Life,* written in 1600 by the philosopher and politician Henry Cuffe, regarded Paracelsus' ideas on longevity as an example "of his either false ostentation, or extreme madnesse." [100] On the other hand, William Vaughan's *Directions for Health* (1600) transcribed many medicines from Hester's translations of Paracelsus.[101] And for Gabriel Harvey the Swiss medical heresiarch was "vir sagacissimus naturae, artisque secretarius," infused with a divine madness.[102]

Although the period of this essay ends with the sixteenth century, we may be curious to glance forward briefly into the opening decades of the seventeenth. The battle lines did not change substantially. The fight for Paracelsan metaphysics and medical philosophy remained clearly lost, despite the efforts of Thomas Timme, whereas that for Paracelsan chemical pharmacology became even more clearly won. On the latter point, perhaps the finest Paracelsophile work was Angelo Sala's discourse on opium, *Opiologia* (1618). The controversy between Matthew Gwinne of the College of Physicians and Francis Anthony of the chemists on the question of the efficacy of the latter's aurum potabile [103] revolved mainly on this issue. Such an outstanding surgical work as John Woodall's *The Surgions Mate* (1617) was decisively Paracelsan.[104] On the other side, the *Chirurgicall Lectures* (1635 and 1638) of Alexander Read of the College

[97] Ed. by G. B. Harrison for Bodley Head Quartos, p. 23ff.

[98] *The Terrors of the Night* (1594, ed. McKerrow) 1. 366.

[99] Act II, sc. 3 L 11.

[100] P. 71.

[101] P. 66.

[102] Gabriel Harvey's *Marginalia* (ed. J. C. Moore Smith) pp. 131, 119.

[103] *Francis Anthony, Medicinae chymicae, et veri potabilis auri assertio* (Cambridge 1610) relied on all prior alchemical and Paracelsan authorities. Mathew Gwinne, *In assertorem chymicae, seu verae medicinae desertorem, Fra. Anthonium* (1611) actually proposed that the books of chemists like Du Chesne, Libavius, and Palmarius be prohibited in England. Anthony retorted with *The Apologie of Aurum Potabile* (1616). John Cotta also took the field against Anthony with *Cotta contra Antonium* (1623). Cotta's *Short Discoverie of Ignorant Practisers* was anti-chemical; see chap. 4 of the 1619 edition.

[104] E.g. pp. 13, 37, 40, 45, 224, 228, 270, etc. See especially the verses at the end of the volume.

of Physicians were loudly Paracelsophobe on all points of theory, however quietly they endorsed the Paracelsan laudanum, stiptic plaster, and similar preparations.[105] I cannot find that William Harvey's work on the circulation of the blood was affected either for better or for worse by the new movement.[106]

REFLECTIONS

When the balance sheet is drawn up for Paracelsism in sixteenth century England one of the chief items to its credit is that it helped to bring medical men back to nature. The worst defect of Galenic medicine at this period was its failure to keep perpetually in touch with fact by physical verification and experiment. It had withered into a mere system of book learning. Stressing the imperative need for more experience, Paracelsus urged the medical man to go out on his travels to study diseases under all conditions, and not to be afraid to dirty his hands in the furnace working out new medicines. This was much to the good, although it is well to remember too that a movement back to the laboratory was already in progress when Paracelsus came to give it fresh impetus. Probably the new chemical medicines would have gone just as far in England, though perhaps not quite as fast, if he had never appeared on the scene. Additionally, however, Paracelsus did medicine the great service of challenging Galenism not merely in pharmacy, but all along the line of medical theory. Such challenges were being directed against the old medicine from many quarters, notably in anatomy and physiology, but the peculiar function of Paracelsus was to propose a whole alternative system and to propose it in so resounding a manner that nobody could help paying attention. He was thus an epitome and rallying point of the diverse forces rising against the old authority in medicine. And although he did not succeed in getting his own theory adopted he prepared men's minds

[105] *The Chirurgical Lectures of Tumors and Ulcers* (1635) 23-5, 102, 128, 198. *Treatise of First Part of Chirurgerie* (1638) 114, 120, 122, 132, 221, etc.

[106] Other Paracelsan or semi-Paracelsan works of the period were Thomas Russel, *Diacatholicon Aureum* (1602); Peter Turner, *Opinion Concerning Amulets* (1603); *Philiatros* (1615); the books of Thomas Timme; Edmund Gardiner, *The Triall of Tabacco* (1610); the 1633 edition of Thomas Vicary's *English Mans Treasure;* and especially William Gordon's *Pharmaco-Pinax* (1625).

Anti-Paracelsan were John Oberndoerffer, *The Anatomyes of the True Physition* (1602); and Thomas Walkington, *The Optick Glasse of Humors* (1607).

for the innovations to come more quietly and more permanently from others.

But in some respects Paracelsan theory was also a liability to medicine. It was highly animistic, filled with all sorts of semi-rational beings like the Archeus who was supposed to govern digestion in the stomach, populated with spirits drawn from neo-Platonism, Cabalism, and other occult lore, committed to sympathetic magic and the doctrine of signatures, dedicated often to the pursuit of miraculous cures, arcana, formulae to produce eternal life, and all expressed in language which seems sometimes intentionally, sometimes unintentionally, obscure—bad either way. Paracelsism therefore tended to import supernaturalism and mystical allegory more intimately into the science of medicine than had ever been the case before. Now English Galenic medicine, on the contrary, had largely succeeded in eliminating the supernatural from practical consideration by the time Paracelsism was becoming widespread in the 1570's. The more plodding, unimaginative Galenists were actually finding the true path of advance in this particular aspect. As pointed out earlier, they had in fact struggled long and arduously against the use of charms, incantations, sympathetic medicine, and similar mumbo-jumbo by the charlatans of the profession. The truth seems to be that Paracelsism was a godsend to these quacks. It gave them a lovely incomprehensible vocabulary and a set of cheap, easily transported medicines for which they could make the most extraordinary claims, with full support from the authority of Paracelsus himself. All the right was not, therefore, on the side of the Paracelsans when the orthodox school accused them of leading a retrogression in medical practice.

Even in the matter of specific chemical remedies, where Paracelsism made its most lasting contribution, the benefit was by no means unadulterated. Powerful inorganic chemicals rashly administered by untutored empirics undoubtedly killed their thousands. Even when administered by conscientious and reputable physicians they probably killed other thousands, because in the absence of controlled experiments no organized body of information had yet been built up which would tell the practitioner what doses of what substances were safe under what conditions. Galenists in their bitter objections were of course motivated partly by professional jealousy, sheer animal inertia, and the like, but it would not be fair to deny them also a genuine concern for the lives of their patients. Further-

more, the search for a miraculous essence of herb or mineral which would cure all diseases, or most diseases, at a single blow seems to have taken up much of the energy of even the more competent Paracelsans, and must have done a good deal to discourage that patient study of individual remedies along which lay the true course of medical advance. To be sure, not all followers of Paracelsus held this extreme view, or, if they did, abstained from broader chemical experiment. Paracelsus himself had devised innumerable cures for particular ills. But one tendency of his influence was to encourage both the genuine visionaries with their balsams and elixirs, and the quacks with their diacatholicon aureum and whatnot who sought a quick way to maximum profit.

Whether, after all this, the good or the bad predominated in the final outcome is a question to which no simple answer can be given. The immediate effect on the patients of Paracelsan practitioners may well have been net loss. Personally, I would rather have had a Galenist than a Paracelsan as my physician in the year 1600. But in the long term, Paracelsism undoubtedly served the cause of progress well by its indirect results in encouraging new research and especially a new, more receptive attitude of mind. By such mixed blessings science struggles forward.*

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A Note on William Hunter and Tobias Smollett

JANE M. OPPENHEIMER*

TOBIAS SMOLLETT'S early career was in many respects curiously parallel to that of William Hunter. Smollett was born in 1721, three years after Hunter, in Dumbartonshire, a county adjacent to Lanarkshire where the Hunters were born. He became apprentice to a Glasgow surgeon and started to study at the University of Glasgow during the very years when Hunter was a student in the same University. Smollett went up to London in 1739, and the following year became surgeon's second mate in the Navy, by a process duly described in the *Adventures of Roderick Random;* Hunter, near the start of his own professional career in London a few years later, was lecturing on surgery to a group of naval practitioners.

Smollett was a friend and pupil of the influential obstetrician, William Smellie; Hunter lived in Smellie's house when first he came to London. Smollett was residing and presumably practicing surgery in the late John Douglas's house in 1744; Hunter was then making his home with the family of the late James Douglas, John Douglas's brother, and was formulating plans, never consummated, to succeed along with John Douglas's son-in-law to John Douglas's surgical practice. Smollett took the degree of Doctor of Medicine at the University of Glasgow in June 1750;[1] Hunter received his degree from the same institution in October of the same year.

It was inevitable that two men of such similar background and common interest should move into conjunction with one another, and accordingly Hunter and Smollett became not only acquaintances but friends. It is not clear when they first met, but they presumably knew each other before the death of Hunter's brother James in 1745, for James

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[1] Smollett in a letter says 1749 (Tobias Smollett, *Letters of Tobias Smollett, M.D.*, edited by E. S. Noyes, Cambridge, U. S. A., 1926, p. 80). All of his biographers give the date as 1750.

himself had been a friend of Smollett's: a letter of Joanna Baillie's[2] spoke of James as "a man of the most promising abilities, handsome in his person, and so engaging in his manners that his friends—amongst whom were Dr. Smollett and Dr. Cullen—always spoke of him with strong marks of attachment and admiration."[3] One of Smollett's early biographers, Anderson, states too that William Hunter and Smollett were friends during the 40's: writing of the novelist's associates in this decade he specified that "among his countrymen of the medical profession he was so fortunate at this time as to be cordially connected with Dr. Clephane,[4] Dr. Macaulay,[5] Dr. Dickson,[6] afterwards physician to the London Hospital, [and] Dr. Hunter."[7]

The degree of "cordiality" characterizing Smollett's connections with Hunter in these early days is rendered questionable, however, by a letter Hunter wrote to his former teacher Dr. Cullen the 29th July 1758.

> Smollett I know not what to say of. He has great virtues, and has a turn for the warmest friendships. I have seen him very little for some years. He is easily hurt, and is very ready to take prejudices. There had been a great shyness between him and me, which his very kind behaviour to me when I was attacked by Douglass, Pott, and Monro,[8] has as yet scarcely conquered, so that I cannot well say how you stand with him; but if I can make you friends I will most certainly.[9]

The earliest that Smollett could have behaved kindly towards William in connection with the attacks by Pott and Monro was around

[2] The poetess and dramatist, Sir Walter Scott's confidante and correspondent, the "immortal Joanna." She was the daughter of William Hunter's sister Dorothea Hunter Baillie, and the sister of Dr. Matthew Baillie.

[3] Stephen Paget, *John Hunter, Man of Science and Surgeon (1728-1793)* (New York 1897) 29. Original letter in the possession of the Royal College of Surgeons of England.

[4] John Clephane, M.D. St. Andrews, F. R. S., Physician to St. George's Hospital. He was an intimate friend of David Hume's.

[5] George Macaulay, M.D. Padua. He moved to London only in 1752 according to William Munk, *Roll of the Royal College of Physicians of London* (2nd ed., London 1878) 2. 181. He was Physician and Treasurer to the Lying-In Hospital, and husband of the historian Catharine Macaulay.

[6] Thomas Dickson, M.D. Leyden, F. R. S., Physician to London Hospital. His wife was a sister of William Hunter's friend the Reverend Alexander Carlyle.

[7] Quoted by John Glaister, *Doctor William Smellie and His Contemporaries* (Glasgow 1894) 115.

[8] With Pott over priority for the elucidation of the nature and origin of congenital hernia, and with Alexander Monro *secundus* over priority for the discovery of the lacrimal ducts and the seminiferous tubules in man and for an explanation of the absorptive functions of the lymphatic system. For discussion of the former, see Fenwick Beekman, "The 'Hernia Congenita' and an account of the controversy it provoked between William Hunter and Percivall Pott," *Bull. N. Y. Acad. Med.*, 2nd series, 22 (1946) 486-500.

[9] John Thomson, *An Account of the Life, Lectures and Writings of William Cullen, M.D.* (Edinburgh 1832, 1859) 1. 549.

1756 or 1757 when Smollett's *Critical Review* was pleading William's case in the controversies. If Smollett's shyness towards Hunter was not yet conquered when William wrote of it to Cullen in 1758, it must surely have been overcome shortly after. It was in 1762 that William published his *Medical Commentaries, Part I. Containing a plain and direct answer to Prof. Monro, jun.,* which Smollett is said to have helped him write; Smollett's *Critical Review* reviewed unfavorably a criticism of Hunter in 1763, and the following year Hunter published the *Supplement to the first part of the medical commentaries* with which Smollett also presumably helped him.

It is, furthermore, a matter of record that the personal relationships between William Hunter and Smollett grew warm with the years. Smollett wrote to Hunter in 1762 first asking financial assistance, and later acknowledging it with gratitude. He wrote to him in June 1763: "I seize this opportunity of thanking you for the manifold Instances of your Friendship which I have received; Instances which I shall ever remember with the strongest emotions of Gratitude, Esteem & Affection,"[10] and in August the same year: "I received your obliging Letter in due Course; and the Contents of it are engraven on my Heart."[11] The following year the novelist addressed to Hunter a letter of tremendous proportions from Nice, describing in equal detail the symptoms of his own malaise and his traveler's experiences in Montpellier and Nice.[12] In 1766 Hunter wrote on the back of a letter from Smollett promising formally to repay his loan of £50, "In case of my death I desire my Executors will not make any demand upon Dr. Smollett, because I sent the money to him as a present, never meaning to take it again," [13] and the following year Smollett addressed him a letter[14] which though full of expressions of his own misery, somatic and psychic, still poured out gratitude to Hunter.

* * * * * *

Financial generosity was hardly one of Hunter's major virtues. His friend Simmons described, for instance, how James Douglas the younger,

[10] Smollett, *op. cit.* 83. Original letter in the possession of the Royal College of Surgeons of England.

[11] *Ibid.* 87.

[12] *Ibid.* 88-94. Original letter in the possession of the Royal College of Surgeons of England.

[13] Victor Plarr, "Unpublished Letters to William Hunter," *Chambers's Journal* 9 (1906) 57. Original letter in the possession of the Royal College of Surgeons of England.

[14] Smollett, *op. cit.* 101-103. Original letter in the possession of the Royal College of Surgeons of England.

son of Hunter's friend of the same name, "who set out in life with the fairest prospect, ruined himself by his indiscretion, and died about the year 1755, aged 30 years. It has been injuriously reported of Dr. Hunter, that he suffered his friend's son to languish in poverty, without administering to his wants. The truth is, that Dr. Hunter, after lending him at different times a larger sum than he could conveniently spare, was obliged to abandon him to his imprudence. Mr. Douglas's notes of hand to the amount of about 100 l. are in the possession of Dr. Hunter's executors." [15] No doubt it was injurious to William that such a report circulated; but surely one hundred pounds is not a fortune to have squandered on the son of a man from whom William would have accepted all he would have offered in terms of hospitality and training, and on the brother of his fiancée.[16]

There is no question but that Hunter valued money highly; he was frank to admit it. He wrote to Cullen, for instance, on 22 February 1752: "Physic is in a strange ferment here. The practitioners in midwifery have been virulently attacked, but by a madman; and in that scuffle I have had a blow, too, obliquely;—the reason is, we get money, our antagonists none. May the dispute, therefore, long continue."[17]

> When first he began to practice midwifery, [wrote Simmons] he was desirous of acquiring a fortune sufficient to place him in easy and independent circumstances. Before many years had elapsed he found himself in possession of a sum adequate to his wishes in this respect, and this he set apart as a resource of which he might avail himself whenever age or infirmities should oblige him to retire from business. I have heard him say, that once he took a considerable sum from this fund for the purposes of his museum, but that he did not feel himself perfectly at ease till he had restored it again.[18]

Such preoccupation with the distribution of his earnings bordered perhaps on the pathological. Samuel Johnson considered it to have done so, as we can detect from a comment buried in Boswell's *Life of Johnson.* Boswell, writing of Johnson, on 30 March 1783 said:

[15] Samuel Foart Simmons, *An Account of the Life and Writings of the Late William Hunter* (London 1783) 8, fn. Simmons, M. D. Leyden, F. R. S., was Physician to Westminster Infirmary and St. Luke's Hospital, Physician Extraordinary to George III, and for many years editor of the *London Medical Journal.*

[16] William Hunter, who never married, had been engaged to James Douglas's daughter, who died during their engagement in 1744 at the age of 28.

[17] Thomson, *op. cit.* 1. 544.

[18] Simmons, *op. cit.* 56.

I found him at home in the evening, and had the pleasure to meet with Dr. Brocklesby. . . .[19] He mentioned a respectable gentleman who became extremely penurious near the close of his life. Johnson said there must have been a degree of madness about him. "Not at all, sir (said Brocklesby), his judgment was entire." Unluckily, however, he mentioned that although he had a fortune of twenty-seven thousand pounds,[20] he denied himself many comforts, from an apprehension that he could not afford them. "Nay sir (cried Johnson), when the judgment is so disturbed that a man cannot count, that is pretty well."[21]

This conversation took place the day of Hunter's death; and that it was in fact Hunter who was spoken of is made clear by the editor of Johnson's *Letters,* who wrote:

The following curious manuscript note in my possession, which is, no doubt, Dr. Brocklesby's record of the conversation of this evening, shows that the respectable but penurious gentleman was the famous physician, William Hunter.

"At Dr. Johnson's, with J. Boswell, Esq., 30th March, 1783, when Dr. Hunter dyed. . . . Dr. Hunter fell a sacrifice to his last Lecture—spent £100,000 on his Collection, nothing on Himself."[22]

* * * * * *

Whether out of folly or frugality, it is undeniable that Hunter guarded his purse-strings jealously. The very fact, therefore, of his presentation of fifty pounds to Smollett is a measure of his benevolence towards the novelist, though to be sure he might have acted with more unostentatious charity had he destroyed Smollett's note instead of endorsing it. It is possible, however, that he was happy to be of service to Smollett not only as to a friend in need but as to a worker in a field other than his own who shared with him a common goal.

Smollett was focussing attention, in his vivid and pointed satires,[23] on the necessity for improvement of medical and surgical education and practice; Hunter was devoting his energies to instituting the needed reforms by introducing into England enlightened and modern methods of

[19] Dr. Richard Brocklesby, a fashionable physician distinguished for his generosity and public-spiritedness. In 1788 he gave Edmund Burke £1000 and an offer to repeat the gift "every year until your merit is rewarded as it ought to be at court" (Fielding H. Garrison, *An Introduction to the History of Medicine* [4th ed., Philadelphia 1929] 391). He wrote a fine treatise on military hospitals and camp diseases, and a dissertation on the music of the ancients.

[20] The correct figure was £19,000, *including* £8000 for the support of the Museum.

[21] James Boswell, *Life of Johnson* (ed. by Augustine Birrell, London 1896) 6. 14.

[22] Samuel Johnson, *Letters of Samuel Johnson, LL. D.* (ed. by George Birkbeck Hill, Oxford, Clarendon Press 1892) 2. 437.

[23] For an excellent analysis of Smollett's critiques of the status of medicine and surgery, see Claude E. Jones *ed.*, "An Essay on the External Use of Water, by Tobias Smollett," *Bull. Hist. Med.*, 3 (1935) 31-82.

medical teaching. It may have been mere coincidence that these two Scots, so comparable in origin and training and so closely allied for many years, were attacking the same evil, each with his own weapon of predilection; it would seem more likely, however, that their cooperation was deliberate, and fostered by some more immediate contingency than a simple realization of the deficiencies in current practices. For surely these were obvious to many men, both medical and non-medical, although few took the trouble to attempt their correction. Hogarth made an attempt comparable to Smollett's, to be sure. But he could delineate only the results of the system he was endeavoring to portray; Smollett, with the benefit of his expert training, could expose as well its working defects. That Smollett chose to do so was certainly in part a result of his natural inclinations; but it is not impossible that he was encouraged by Hunter to follow them out. For Hunter may well have envisioned, in his friendship with Smollett, opportunity to expand his own sphere of influence, and to exert his pressure indirectly, through the creative writings of his friend, as well as directly through his own teaching.

On Abraham Flexner's Eightieth Birthday

ALAN GREGG*

SPEAKING OF ST. BASIL, St. Gregory of Nazianzus some 1550 years ago asked, *"Who went deeper into the mysteries of philosophy, both that high philosophy which holds it face upward toward the sky (theoretica) and that which is concerned with the daily actions of life (practica), as well as that third kind which deals with demonstrations, oppositions and arguments and is called diaectic? . . . he obtained such an insight that even with the best he could hold his own, and with medicine both theoretical and practical he made himself thoroughly familiar."*[1]

"That third kind which deals with demonstrations and argument and is called dialectic" was just what President Henry S. Pritchett of the Carnegie Foundation for the Advancement of Teaching sought to apply to the state of medical education in the United States in 1908. He found it in Abraham Flexner, who had the other characteristics attributed to St. Basil, and the first result in 1910 was the Report on Medical Education in the United States, a cardinal document in the history of medical education in the present century.

With characteristically candid modesty Mr. Flexner once told me that credit for two crucial decisions in his life should be given to others. It was his wife who insisted on his giving up his extraordinarily successful private school in Louisville and putting in the year at Harvard which resulted in his book on the American College. And it was Dr. William H. Welch who suggested the tactics to be employed in the Carnegie Report on Medical Education—a thorough exposition of observable or admitted facts, without glosses and without omissions. The result was as incontrovertible and as convincing as the record of a necropsy. It was reminiscent of a Dun and Bradstreet report or of the advice that "adjectives are like leaves on a branch: they may make it beautiful but they ruin it for switching purposes." Indeed the stark facts of the Flexner Report were so eloquent as to alienate the sympathies of Mr. Carnegie himself from any attempt to rectify the training of a profession so glaringly remiss in its educational responsibilities. At about that time Wallace Buttrick and his General Education Board recognized how remarkable was the opportunity of using Abraham Flexner's abilities in behalf of medical education. The appointment of Mr. Flexner as Secretary of the General Education Board marked the beginning of a second phase in

*The Rockefeller Foundation.

[1] Or. XLIII, 23, quoted by E. K. Rand, *Founders of the Middle Ages* (Harvard Univ. Press, 1929) 227.

the service of a Louisville private school teacher to medical education. "Can any good thing come out of Nazareth?"

The order of St. Nazianzus' stipulation that dialectic deals with "demonstrations, oppositions, and arguments" needs only to be reversed to describe the sequence of Flexner's experience in the General Education Board. He first prepared his arguments with ample evidence and insistent logic. The essence of the arguments was that medical education must be dealt with as a form of education, not as a sacrosanct or mysterious apprenticeship, or an unsupervised initiation into an inviolable guild. This argument was the heart of the *theoretica*: one of the *practica* was the full-time principle. Of course the oppositions centered around the principle of full time as applied to headships of clinical departments in medical schools. The opposition was often forthright but just as often evasive or intransigent. It is doubtful if even Abraham Flexner's fearlessness, skill in argument, and tenacity of purpose would have prevailed over the oppositions had it not been for the funds given by the General Education Board for demonstrations. Full time was expensive—extremely expensive—but Flexner successfully argued before his Board that its advantages would justify what were considered enormous outlays for endowment. Over all the General Education Board "invested" over $85,000,000 in the medical schools of the United States, with the constant objective of improving the educational or pedagogic aspects of medical education. And these large gifts by their boldness and by the clarity of the idea they incarnated attracted a measure of financial support from private donors that was unparalleled in American higher education.

This is not the place for a detailed discussion of the evolution of full time teaching in medical schools. But perhaps a reasonable summary would be this: full time, like a plaster cast, was applied in overcorrection of the abuses of part-time teaching. Some pressure sores developed, but when the casts were removed the functional results were excellent. Or, to avoid argument by analogy, it may be said, and almost certainly, that if the financial burden of full time can be met, those who have experienced it will not forsake it or wish to see it given up.

I would repeat the full-time principle was not the essence of Abraham Flexner's great contribution to medical education in America and throughout the world. Full time was only one of the embodiments of a far deeper and more promising concept: medical education must be understood and practiced in terms of *education*. The implications of that concept remain for this and subsequent generations to put into practice. That task is not completed—indeed at times it seems hardly more than begun.

Abraham Flexner's eightieth birthday on November 13th, 1946 marked an occasion for admiration and gratitude. Admiration for his perpetual devotion to excellence, his superb dialectic skill, his power of analysis and lucid exposition, and his realism, unsparing of blame and of praise. And gratitude that he has been granted eighty years to lavish these talents upon medical education, relentlessly and successfully our teacher.

Etiological Theory in America Prior to the Civil War

PHYLLIS ALLEN*

I

DURING the nineteenth century American medicine[1] began to emerge as a science. This development was a reflection of a similar trend in Europe, rather than an indigenous affair. New methods were introduced which made it possible to extend knowledge appreciably. Pathological anatomy was imported from France during the twenties, and at about the same time European-trained Americans began to adopt a more exact system of clinical diagnosis and observation, so that with the combination of the two methods, it became possible to differentiate between diseases which heretofore had been lumped together in the amorphous term "fever." With the new knowledge, William Gerhard of Philadelphia, who had been a pupil of Louis in Paris, was able to distinguish between typhoid and typhus fevers in 1837.

In the early part of the century, new specialized auxiliary sciences were developing, which suggested different paths for medical men to explore. As John K. Mitchell put it in 1850: "Medicine has become a science. It could not be so much before our day. The sciences upon which it is built were themselves too imperfect to afford it a serious basis until now."[2]

Pathological anatomy and the new clinical methods together helped stir up a renewed interest in etiology. Man has been interested in preventing and controlling disease since ancient times. There has always been a universal desire to stop epidemics, but in order to ward off disease it was first necessary to know the causes. It is not surprising, then, to find that a moderate interest in etiology was common to all times. Noah Webster, who made a careful study of the literature on the "Cause and Origin of

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[1] The author is greatly indebted to Dr. Richard H. Shryock, of the University of Pennsylvania, for advice and encouragement in making this study.

[2] John K. Mitchell, *Impediments to the Study of Medicine. A Lecture Introductory to the Course of Practice of Medicine* (Philadelphia 1850) 21.

Pestilence" since the days of Hippocrates, was struck by the volume and diversity of opinion.[3]

During the early nineteenth century, this interest in etiology greatly increased, especially during the 1840's.[4] For a long time a great controversy had been raging in the academic world between the contagionists and the non-contagionists. The chief disease involved was yellow fever, though other fevers were also included. The contagionists believed it to be of imported or transported origin, spread by people or animals, while the non-contagionists thought it of spontaneous or imported origin, spread by infected air. Some doctors were contagionists in respect to one fever and non-contagionists in regard to another. Schools of thought arose on the subject, and in 1805, "the College of Physicians of Philadelphia, as a body, gave their opinion in favour of contagion" in the case of yellow fever—a view which failed to settle the matter.[5]

The arguments in America were a reflection of those in Europe, particularly in England, and on both continents, logic, backed up by statistics from a scattering of cases, was the chief means of proof. An accurate knowledge of the causes of disease would settle the problem, so the adversaries were constantly looking for more information to bolster up their reasoning. Such satisfaction with observation and reason instead of experimental method held back the development of medicine as a science.

[3] Noah Webster, *A Brief History of Epidemic and Pestilential Diseases* (London 1800), I, Sect. I, 1-26.

[4] An analysis of the sources used in this study shows a spread as follows:

1700-1800	12	1830-1840	14
1800-1810	7	1840-1850	28
1810-1820	7	1850-1860	19
1820-1830	12	1860 ff	4

[5] Sir Gilbert Blane, *Select Dissertations on Several Subjects of Medical Science* (London 1822), Pt. IX, "On the Yellow Fever," 321. See also Robert Jackson, *A Sketch (Analytical) of the History and Cure of Contagious Fever* (London 1819), *Outline of the History and Cure of Fever, Endemic and Contagious . . .* (Edinburgh 1798); David Hosack, *Essays on Various Subjects of Medical Science* (New York 1824) I; William Currie, *A Treatise on the Synochus Icteroides or Yellow Fever as it Lately Appeared in the City of Philadelphia . . .* (Philadelphia 1794), *Observations on the Causes and Cure of Remitting or Bilious Fevers* (Philadelphia 1798); William Cullen, *First Lines of the Practice of Physic* (1794); Meredith Clymer, *Fevers: Their Diagnosis, Pathology and Treatment* (Philadelphia 1846); Elisha Bartlett, *The History, Diagnosis and Treatment of Typhoid and of Typhus Fever; with an Essay on the Diagnosis of Bilious Remittent and Yellow Fever* (Philadelphia 1842), *The History, Diagnosis and Treatment of the Fevers of the United States* (Philadelphia 1847); Benjamin Rush, *Medical Inquiries and Observations* (Philadelphia 1805), IV; Samuel Jackson, *An Account of the Yellow or Malignant Fever as it Occurred in the City of Philadelphia in 1820* (Philadelphia 1821); Edward Nathaniel Bancroft, *Essay on the Yellow Fever* (London 1811), *Sequel to an Essay on the Yellow Fever* (London 1817); Noah Webster, Jr., *A Collection of Papers on the Subject of Bilious Fevers Prevalent in the United States for a few Years Past* (New York 1796), *Brief History of Epidemic and Pestilential Diseases;* Edward Miller, *Medical Works* (New York 1814).

As it became obvious that fevers were not a unity as Benjamin Rush and others had believed, attempt was made to classify the miasmatic influences which were believed to be the causes of disease. Further impetus was given to this movement when pathological anatomy showed that different results were found in autopsy after different kinds of fever. There were no hard and fast lines between the various types of fevers until the work of Gerhard became known in the 1840's. The general classification of intermittent, continued, and eruptive fevers was then seen to be inadequate. Nosologists had attempted to describe diseases on the basis of symptoms alone, but they soon had so many that even those opposed to the idea of the unity of disease became skeptical. Simple systems were devised by William Cullen, who divided his fevers (Pyrexiae) into four classes: Febres, Phlegmasiae, Exanthemata, and Haemorrhagiae; and Noah Webster, who, though not a physician, worked out a system based on exhalation in healthy and pestilential periods.[6] Later more elaborate systems were evolved from these simple ones.

The most urgent reason for the increased interest in etiology was the introduction of pathological anatomy. This specialty led to a differentiation among fevers for anatomical reasons. For example, characteristic lesions in the intestine of a corpse enabled the pathologists to tell that a person had had typhoid fever. Then these findings were correlated with the clinical symptoms and eventually an accurate picture of the disease was obtained which enabled the medical profession to see it apart from other types of fever. *Differentiation of typhoid, typhus, malaria, and yellow fever as separate diseases made it necessary to hunt for separate causes in the external environment.* The idea that typhoid or yellow fevers were an advanced form of malaria (bilious fever), was gradually replaced by the idea that these were different diseases. On the other hand, this development in etiology could not take place until the character of the disease was clear. Obviously the pathological differentiation in diseases made the old idea of the unity of disease obsolete. It was now impossible to ascribe all fevers to the same cause.

Finally the increased interest in etiology was fostered by the development which was taking place in the sciences allied to medicine. New discoveries in the specialized fields of mycology, parasitology, physiology,

[6] *The Works of William Cullen, M. D.* (ed. John Thompson, London, 1827) 1. 243; Webster, *Brief History* 2. 339ff.

pharmacology, chemistry, cellular pathology, and other sciences had their applications in the field of medicine. Sometimes the new discoveries could be applied directly, sometimes they suggested new solutions to old problems by analogy. The relationship between mycology and etiology, for example, becomes apparent in the work of John K. Mitchell on the fungus origin of disease.

II

The European influence on American medicine during the period under discussion should not be underestimated. European scientists were very much interested in etiology and many theories were developed to account for the various types of diseases. The most common of the prevailing theories were those based on marsh miasma, atmospheric causes, chemical causes, electricity, predisposing causes in the individual, fomites, and animalculae. In general the causes of disease were divided into two types: proximate causes, which included pathological changes and symptoms within the body, and remote causes, which covered the external factors of predisposition and exciting or specific causes. Some of the reasons given for the existence or development of disease now appear absurd, but in an age which was groping in the dark anything seemed rational. In fact, some of the ideas which led to present day knowledge seemed so irrational that only the more daring scientists included them.

The theory most accepted for centuries was the one which ascribed fevers to the action of a volatile poison arising from the marshes called *miasma.* This noxious effluvium in the air was variously assigned to decaying vegetable matter, to stagnant water, to decaying bodies on battlefields, or to plain putrefying animal matter. Serious debates were held over which of these matters was the actual cause of the *mal aria.* Heat, moisture, and dead organic matter were generally agreed to be the agents in the propagation of the miasma. Some authors even went so far as to determine the exact degree of temperature needed. All sorts of diseases were thought to be caused by the action of malaria. John Macculloch scarcely skipped any disease known to man in his list. Charles Caldwell believed that miasmata were responsible for plague, yellow fever, cholera, bilious fevers (intermittents, remittents, continued fevers, dengue, dysentery, and lesser disorders), madness, and idiotism. He held that "bilious malaria" arose from vegetable and animal matter, especially the former, in a state of dissolution. A. P. Wilson Philip divided his miasmatic fevers

into intermittents and continued fevers, the former caused by marsh miasma as well as continued exposure to dampness, especially at night, and the latter due to cold, and contagion by means of direct contact, fomites, and infected air. Benjamin Rush even enumerated which kind of vegetable and animal matters gave rise to poisonous exhalations. He also noted the importance of stagnant water in hogsheads, gutters, mill ponds, puddles, and sinks, and the stagnant air in the hold of a ship or a close cellar.[7]

There were several well-known facts about disease presumably produced by miasmata that were not explained by this theory. One notable exception was that sometimes cases of fever were found in dry uplands, far away from any swamps; another was the "fact" that trees seemed to protect residences from the effects of bad air; in addition, the *mal aria* was stopped by so fine a barrier as a piece of gauze; it could not proceed against the wind; fire and smoke dispersed it, and although it could not travel very far over sea water, sporadic cases often appeared on shipboard. M. L. Knapp's comment illustrates the problem:

> Malarious fevers occur at sea, are produced on ship-board, where there can be no marsh malaria. Also in arid deserts, where no vegetable or animal decomposition is going on. Also in the cool dry mountains of California where never a marsh or lake existed, where no rain falls for six months of the year, and no vegetation, comparatively, is produced, subject to decay. One such example is fatal to the hypothesis of malaria, and a dozen may be cited.[8]

All this questioning led to a casting about for better theories, yet none of them satisfied the searchers. Caldwell, in the Boylston Prize Essay for 1830, asked and answered the basic question:

> 1. *What is the* NATURE *of the malaria that produces bilious fever?*
> To this my answer is brief. *I do not know*. Nor is anyone better informed about it than myself.[9]

[7] John W. Monette, "Observations on the Epidemic Yellow Fever of Natchez and of the South-west," *West. Jour. Med. Surg.* 4 (Sept. 1841) 172; Usher Parsons, *On the Comparative Influence of Vegetable and Animal Decomposition as a Cause of Fever* (Philadelphia 1830) 13; Anonymous, "Book Review," *Amer. Quart. Rev.* 4 (Dec. 1828) 288 n.; Charles Caldwell, *Essays on Malaria and Temperament* (Lexington, Ky. 1831) 17 ff.; A. P. Wilson Philip, *A Treatise on Febrile Diseases . . .* (2nd ed., Hartford 1816) 74 ff., 149 ff.; Benjamin Rush, *Medical Inquiries and Observations* 4. 163 ff.

[8] M. L. Knapp, *Inquiry into the Nature of the Nursing Sore Mouth Affection* (n.p. 1856), Ch. III, "Etiological Deductions—Malaria," 261.

[9] Caldwell, *Essays on Malaria* 35; Charles J. B. Williams, *Principles of Medicine, comprising General Pathology and Therapeutics* (Philadelphia 1844) 73-74; R. S. Holmes, "On Malaria in Connection with Medical Topography," *St. Louis Med. Surg. Jour.* V, No. 6 (May 1848) 532.

This same answer was to be given for the next fifty years. Charles J. B. Williams, in 1843, foresaw the fact that "the microscope rather than chemical analysis may be expected to discover the nature of malaria." R. S. Holmes, in 1848, was convinced that the discovery of "what malaria really is" was just around the corner.[9]

Those people who believed in the miasma theory found it necessary to differentiate between the various miasmata to account for the different diseases resulting from them. The comparatively simple systems of Cullen and Webster became the complex and exhaustive systems of Edward Miller, Joseph M. Smith, and James Copland.[10] Such fine divisions, with their vagueness and overlapping agencies reached the heights of possibility in the realm of reason and logic, but the problems of etiology were no nearer solution after the lists had been made than they had been before. On the other hand, the results obtained in pathological anatomy made it imperative to attempt some kind of classification. Up to 1865 none of the theories of disease had been satisfactorily proven, and the American medical profession generally accepted the miasma theory as best answering the needs of etiology.

The second most popular theory placed the blame for disease on atmospheric conditions rather than miasmata. This theory was chosen in times of epidemics when the adherents of the miasma hypothesis were at a loss to explain how the poisonous content of the air suddenly increased rapidly without apparent cause. Proponents of the atmospheric theories were able to ascribe this to a variety of cosmic happenings, ranging from comets to an altered pressure or oxygen content. F. A. Mesmer had a magnetic theory based on the action of the planets and stars. The anonymous reviewer in the *American Quarterly Review* for December, 1828, thought that climate determined whether the same disease would be the yellow fever of New Orleans or a mild remittent in Canada. These ideas were not new. Hippocrates and Galen had placed the origin of disease in weather, heat, and the sun. Van Helmont added earthquakes and sieges. Sydenham preferred to believe that some occult properties in the air were responsible for epidemics. In the eighteenth century, Henry Clutterbuck used fumigation to prevent disease, not, as today, to kill the wild life, but to purify the air. Noah Webster included cosmic atmosphere

[10] Cullen, *First Lines of the Practice of Physic*, Part I, "Fevers," 540 ff.; Edward Miller, *Works*, "Nomenclature," 194 ff.; James Copland, *Dictionary of Practical Medicine* (New York 1845), III, "Fevers," 1130-31, IV, "Infection," 404-405 n.

among his causes of disease; he had not only miasmata and noxious gas in his air, but an "electric fire" disturbing it. He also noted a possible correlation between epidemics and comets, earthquakes, meteors, storms, water which has gone bad from a subterranean vapor (hot springs?), fog, and the aurora borealis. Webster's most interesting observation was that "changes in the air" produced myriads of insects before epidemics. He thought that electrical changes in the atmosphere might be the cause of this sudden enlargement in their numbers.[11]

Many persons considered the atmosphere in the nature of a *conductor* of the poisons generated by sick people. Contaminated air would pass the disease along in a confined place. Thus the well received the disease from the sick by being present in the sick room. This is generally what early nineteenth-century writers meant when they mentioned contagion. Robert Jackson gives this view in contemporary medical terminology:

> It is perhaps no longer disputed that those derangements of the human frame denominated fevers, whether endemic or contagious, owe their origin to two sources only . . . [the first is marsh exhalation;] the second, deriving from an animal source, more expressly from an altered condition of the living human body, is confined in its sphere of action, communicated only by contact, by near approach, or by a medium connected with this source.[12]

Hildenbrand called this contagion by immediate contact—meaning by close, infected hot air. He also mentioned furs, dirty bed clothes, and the like as being part of this category of contagion, but he indicated that in such instances contagion would probably take place by inoculation, rather than by respiration.[13]

There was a general belief that the atmosphere became a conductor of disease when it was in a certain putrid or corrupted state. One of David Hosack's correspondents thought yellow fever required such an atmosphere. The contagion would then "inoculate and assimilate any confined portion of atmosphere which has been exposed to the requisite causes of contamination, so that every part of it shall have the power to communi-

[11] *Amer. Quart. Rev.* 4. 299; Henry Clutterbuck, *Observations on the Prevention and Treatment of Epidemic Fever* (London 1819) 57; Webster, *Collection of Papers on the Subject of Bilious Fevers,* 233 ff.; Webster, *Brief History* 2. 120-95.

[12] Robert Jackson, *Outline of the History and Cure of Fever, Endemic and Contagious* 1. 102.

[13] J. Val. de Hildenbrand, *A Treatise on the Nature, Cause, and Treatment of Contagious Typhus,* (trans. S. D. Gross, New York and Philadelphia 1829) 76, 83. Hildenbrand believed that contagion entered the system through the alimentary canal, lungs, abraded skin, and direct contact—a very modern approach.

cate the poison." Thus epidemics could be explained by atmospheric changes alone. Richard Mead in the eighteenth century had believed that a person ill with plague gave off contagious atoms, which would be harmless in a healthy state of air but virulent in a corrupted state. W. C. Daniell of Georgia thought that marsh miasma imparted some property to the air, presumably a pressure change, which impaired the "capillary action of the skin, and affect[ed] the equilibrium of the circulation." This in turn upset the internal capillaries and the net result was fever.[14]

In writing of epidemics, most medical authors began by giving the medical geography of the area and the state of the weather during the few weeks preceding the outbreak of the epidemic. Thus Samuel Henry Dickson reflected that the weather had been changeable before an epidemic started in Charleston in 1827. James Gayley was sure that there was usually a higher temperature in a place subject to malaria. Daniel Blair considered that in yellow fever "the efficient cause . . . is an aërial poison, probably organic, which requires a certain temperature for its generation and existence, and affects special localities and persons." J. W. Heustis noted that in Alabama "the appearance of certain diseases is connected with particular temperatures of the atmosphere. We know, for instance, that a long continuance of hot weather is necessary for the generation and prevalence of the bilious or yellow fever." [15]

Finally there was one school of thought, mentioned by both Charles Caldwell and Usher Parsons, which ascribed malaria to atmospheric moisture alone. An unidentified author quoted by Parsons related:

> Emigrants proceeding to Alabama and other southern regions, from the low countries of Carolina, find no injury from sleeping in the open air, as their custom at night is to build a large fire of logs, and lay themselves beside it, on some part of their baggage. The effect of the fires in destroying malaria is plain, if the fact of its existence depends upon the presence of moisture; for the moisture being evaporated by the heat, the poison is either dispersed with the vapor, or, if separated from it, falls innoxious, and probably inert. It is on the same principle that smoking

[14] David Hosack, *Essays on Medical Science*, I, Appendix, Note I, 359. Letter from Rev. Samuel S. Smith, Princeton, Jul. 24, 1808; Richard Mead, *A Short Discourse concerning Pestilential Contagion* (6th ed., London 1720) 13; W. C. Daniell, *Observations upon the Autumnal Fevers of Savannah* (Savannah 1826) 47.

[15] S. H. Dickson, "Account of the Epidemic which Prevailed in Charleston, S. C. during the Summer of 1827," *Amer. Jour. Med. Sci.* 2. (1828) 64 ff.; James F. Gayley, "On the Etiology of Intermittent and Remittent Fevers," *Amer. Jour. Med. Sci.*, XVII, No. 33 (Jan. 1849) 53 ff.; Daniel Blair, *Report on the First Eighteen Months of the Fourth Yellow Fever Epidemic of British Guiana* [1853]; J. W. Heustis, "Remarks on the Endemic Diseases of Alabama," *Amer. Jour. Med. Sci.* 2. (1828) 27.

segars on the decks of ships is salutory. The heat and smoke keep a dry atmosphere about the uncovered face and the air respired, thus deprived of miasmata, safe.[16]

One medical writer, M. L. Knapp of Illinois, turned violently against "this Italian absurdity of Lancisci" — the hypothesis of *mal'aria.* He thought that cholera, malaria, and most fevers were of scorbutic origin, and he cured them by feeding his patients lemon juice and a proper diet. Considering that malnutrition was a common condition on the frontier, it is possible that many deficiency diseases were being classified as fevers and treated as such. Copland's classification of infectious diseases included pellagra with syphilis, rabies, cow-pox and other "palpable specific contagions," and scurvy with dysentery and ergotism as caused by "unwholesome and poisonous ingesta."[17] All this added to confusion in classifying diseases, let alone determining their causes.

Doctors who were satisfied with neither the miasma theory nor the atmospheric theories, sometimes preferred a chemical theory. Macculloch considered malaria to be a compound made up of nitrogen, oxygen, hygrometric vapor, and a basis of deleterious character—miasma. Today this sounds like ordinary air, plus miasma. To him, however, it was a mysterious compound which could attach itself to solid substances, including furniture, trees, the soil, and gauze window netting. Furthermore he reasoned that it could be chemically decomposed by fire and smoke. John Cooke believed a vapor or gas of miasmatic origin was the cause of fevers. J. B. McCartney based his theory on changes in the oxygen content of the atmosphere. Other hypotheses, principally borrowed from European theorists, blamed bad air on the presence of ozone, or oxidized nitrogen gas (septon), or carbonic acid gas, or nitrous oxide, or sulphurretted hydrogen, or phosphorated hydrogen, or carbonated hydrogen, or sulphurous emanations in general. Some of these chemical theories display a startling ignorance of the basic laws of chemistry. Hoffmann claimed epidemics were due to the lessened elasticity of the air, and Sir James Murray had an electrical theory. Richard Mead in the eighteenth century had put out the idea that the infectious matter of contagion was "perhaps in the Nature of a *Salt,* generated chiefly from the Corruption of a *Humane Body. . . .*" R. S. Holmes explained that "moisture and heat

[16] Caldwell, *Essays on Malaria,* 36 ff.; Usher Parsons, *Boylston Prize Dissertations* (Boston 1839) Ch. 5, 227.

[17] M. L. Knapp. *Discovery of the Cause, Nature, Cure and Prevention of Epidemic Cholera* (Cincinnati 1855), *Inquiry into Nursing Sore Mouth Affection* 267; Copland, *Dictionary* 4. 402-403.

operating on mineral and vegetable products, are necessary to the constitution of malaria." [18]

The importance of pure water was known, at least on a theoretical basis, even before it was possible to examine it microscopically. Stagnant water was to be avoided because it gave rise to miasma. It was presumed to have deleterious qualities because it smelled bad. Even after the work of John Snow, *On the Mode of Communication of Cholera* (1854), doctors were attributing water-borne diseases to aerial miasmata. Both Rush and Caldwell were well aware that wells became contaminated from privies and disease resulted. It is amazing in many instances to find physicians so close to finding the actual specific causes of disease and still never quite realizing them. The putrid water hypothesis gained converts when Magendie claimed to have produced yellow fever in dogs by injecting them with putrid water and putrid matter. This experiment received considerable attention in America during the 1840's, though it is unlikely that the disease produced was actually yellow fever.[19] The experimentation, however, was a step in the right direction, and it is unfortunate that no American appears to have followed Magendie's method.

A common conviction, dating back to the Middle Ages, was that of contagion by fomites. It was believed that infected matter clung to cloth, fur, baskets, bedding and such, and contagious diseases were passed along in this manner. Henry Clark noted that the spread of Ship Fever (typhus) was decreased by the use of hygienic measures, principally keeping the patients and their bedding clean.[20]

One of the minor hypotheses on the causes of disease received little attention through the ages although it later proved to be the correct one. This was the theory that disease was caused by minute animal or vegetable creatures, similar to infusoria, which were generally called animalculae. The animalcular idea dates back to the Romans: Columella, Varro, Lucretius, Pliny, and Ovid. It was suggested again in the sixteenth

[18] Anonymous, *Amer. Quart. Rev.* 4. 294; John E. Cooke, *Essays on the Autumnal and Winter Epidemics* (Transylvania Univ. 1829) 7 ff.; J. B. McCartney, "Cholera and Periodic Diseases," *St. Louis Med. Surg. Jour.* XIV, No. 2 (Mar. 1865) 105-119; Caldwell, *Essays* 36-49; J. K. Mitchell, *On the Cryptogamous Origin of Malarious and Epidemic Fevers* (Philadelphia 1849), Lecture I; Mead, *Discourse on Contagion* 17; Holmes, "Malaria and Topography," *St. Louis Med. Surg. Jour.* 5. 520.

[19] Rush, *Medical Inquiries* 4. 169-70, 385; Caldwell, *Essays* 94; Samuel Forry, "Statistical Researches Elucidating the Climate of the United States and its Relation with Diseases of Malarial Origin; Based on the Records of the Medical Department and the Adjutant General's Office," *Amer. Jour. Med. Sci.* July 1841, n.s., 2. 46.

[20] Henry G. Clark, *Ship Fever, so called; Its History, Nature and Best Treatment* (Boston 1850) 21.

century, by Fracastorius, Cardanus, Fallopius, and De Bonagentibus, and in the seventeenth century was accepted by Hauptmann, Redi, Kircher, and Leeuwenhoek. During the early eighteenth century, Giovanni Maria Lancisi suggested that malaria was propagated by means of the bites of mosquitoes which let their juices into people when they bit them, and he further pointed out that scientists should use the microscope to look for these insects (worms or parasites) in the blood of the infected.[21]

A few years after Lancisi published his theory, two English writers used the animalcular idea to account for contagion in plague and phthisis. Sir Richard Blackmore considered the possibility that microscopic "worms" might cause plague, but he seriously thought that these "worms" were the *effect* not the cause of "pestilential putrefaction." Benjamin Marten suggested self-reproducing animalculae as a cause of consumption, and he listed his predecessors who likewise had preferred that theory of disease.[22]

In the later eighteenth century Nyander and Linnaeus were the foremost proponents of the theory, although the latter was not too positive about the idea. J. E. Wichmann in 1786 described the itch mite which causes scabies, but this discovery made little impression on his contemporaries. James Tytler, an Englishman, wrote *A Treatise on the Plague and Yellow Fever,* which was published for Americans at Salem in 1799. Tytler noted that disease had similar symptoms wherever it was found. Thus smallpox was the same disease in America that it had been in Turkey. Therefore he concluded that each separate disease must have its own *specific contagion.* Tytler seriously considered the animalcular hypothesis as a possibility, though he did not give it precedence over any other theory. His work evidently received some attention in America because Samuel Brown went out of his way to ridicule it. Brown especially disliked the idea that contagion might take place by means of "invisible, imperceptible, specific and immutable" causes.[23]

On the Continent, Robert Jackson was one of the first to open the question during the nineteenth century. He believed that typhus fever

[21] R. La Roche, *Pneumonia: Its Supposed Connection Pathological and Etiological with Autumnal Fevers . . .* (Philadelphia 1854) 318-19; C.-E. A. Winslow, *The Conquest of Epidemic Disease* (Princeton 1943) *passim;* Palmer H. Futcher, "Notes on Insect Contagion," *Bull. Inst. Hist. of Med.* 4 (July 1936) 546-57.

[22] Sir Richard Blackmore, *A Discourse upon the Plague* (London 1721) 35; Benjamin Marten, *A New Theory of Consumptions* (2nd ed., London 1722) 40-47.

[23] James Tytler, *A Treatise on the Plague and Yellow Fever* (Salem 1799) 190; S. Brown, *A Treatise on the Nature, Origin and Progress of the Yellow Fever* (Boston 1800) 93-95.

was contagious by some means which propagated "its kind through the mechanism of animal organism." This substance was then attached to foreign bodies in a manner similar to fomites and carried to distant places and persons.[24] As to the manner of contagion, he added:

> It is evident that a cause which generates its kind, and manifests that kind by an external product, cannot act otherwise than on an organ of excretion. The product is invisible; it must, therefore, be supposed to be manufactured by and to proceed from the minutest of secreting vessels, viz., those which give out invisible exhalation from the external skin or lining of interior canals.[25]

Jackson never went so far as to call his invisible fomites animalculae, but it is clear that where other writers had contagion occur solely by exhalation, he gave his exhalations an organic body.

The Viennese professor, J. V. de Hildenbrand, had stated the germ theory of disease very clearly in his book on typhus fever, which was made available in translation for American readers in 1829. He said:

> Every contagious miasm possesses the properties, 1, of producing a similar virus in the disease which it has occasioned; and 2, of spreading and extending itself ad infinitum, by virtue of this secondary development, that is so long as there exists a matter capable of receiving the miasm, and of producing a new one. Both these properties are similar, by their power of reproduction, to the germs of animals and of plants; but the last property is analogous to the matter of fire, since a single atom of the contagious virus, like a spark, is capable of spreading itself ad infinitum, and of traversing, when unobstructed in its progress, all bodies capable of receiving it.[26]

He also wrote that the contagious matters possess different degrees of volatility, some like the virus of rabies having none at all, and others like scarlatina having a high volatility when suspended in an animal atmosphere.

J. G. Millingen, in an essay on "Generative Animalcules," mentioned that an experiment had been performed by "Gale," who had taken animalculae from a case of itch, kept them alive in a watch-glass culture, and infected himself with them, producing a similar case of itch. Millingen reflected that the animalculae were the cause and not the consequence of disease, and added:

> I repeat it, this subject is replete with interest; and microscopic experiments may some time or other throw a material light on the practice of medicine. Those

[24] Robert Jackson, *Sketch (Analytical) of Fever* 159.

[25] *Ibid.* 160.

[26] Hildenbrand, *A Treatise on the Nature, Cause and Treatment of Contagious Typhus* 73, 75.

substances that are known to destroy the insect that produces the itch, cures the malady. May not this analogy lead to singular results?[27]

No attempt appears to have been made to follow up this foresight by experimentation.

One theorist who received a considerable amount of attention in America was Sir Henry Holland. He thought it entirely possible that minute insect life, invisible to the naked eye, might be the cause of disease. Furthermore he considered that perhaps epidemic cholera was occasioned by swarms of insects going from one place to another, and that the cycles of virulence in this disease might be due to the incubation periods in the reproductive life of the insects involved. He then logically proceeded to the idea that the insects provided a constant reservoir of infection, ready to expand if opportunity presented itself.[28] Holland also advanced the idea that man himself might be the agent of transmission. His combination of man and "insect" or bacilli as the agents in producing cholera might have led to the final solution if he had also been able to add John Snow's pump and complete the cycle by showing it a water-transmitted disease. This compound type of reasoning was to evade the theorists for another fifty years.

Jacob Henle, who wrote an essay, *On Contagions and Miasms,* in 1840, was far in advance of his time. Though he lacked the modern specific terminology, which made it difficult for him to explain his theories exactly, he had a very clear idea of contagion. It remained for his pupil, Robert Koch, to put the theories into practice, with very successful results. Henle noted that a broken skin was needed in the inoculation type of contagion. Writers like Hodge and Meigs, who fulminated against Oliver Wendell Holmes' work on contagion in puerperal fever, missed this point altogether. Henle believed that disease was caused by seeds or germs, which were living animate beings. These entered the system through mucous membranes, the alimentary canal, and broken skin. Inside the body they increased rapidly, a fact verified by the period of incubation after exposure to the disease. Henle thought the germs were probably plant parasites or fungi, of which different varieties produced

[27] Millingen, *Curiosities of Medical Experience* (Philadelphia 1838) 352-53. The "Gale" mentioned was possibly J.-C. Galès of Paris, who wrote *Sur la diagnostic de la gale, sur ses causes, et sur les conséquences médicales pratiques à deduire des vrais notions de cette maladie* (Paris 1812).

[28] Holland, *Medical Notes and Reflections* (Philadelphia 1839) 342-55.

different diseases. Fever was caused by local inflammation, especially in inoculation cases, and by changes in the blood caused by parasites, in the miasmatic types of disease. Henle reversed the usual thinking of the day by stating that putrefaction was caused by infusoria or fungi. He considered epidemic disease the result of localized germs, infecting the air near active infusoria or fungi.

One of Henle's ideas has since come into vogue in connection with viruses. He thought that a strain could gradually be transformed from a mild form to a more virulent one. The example he used, however, shows that he had not yet absorbed the work of Gerhard and others like him who were differentiating specific disease entities: he thought it entirely possible that an intermittent fever could be gradually transformed into a contagious typhoid or yellow fever. This idea does not fit with his previous conclusion that different parasites cause different diseases. Evidently the influence of older accepted theories was still very strong.[29] The work of Henle seems to have had little influence in America, though mention of his *Pathologische Untersuchungen* found its way into the *British and Foreign Medical Review* (1840).

There were many minor figures who held to the animalcular hypothesis, such as the Italians, Mojon, Grassi, Rasori, the Frenchmen, Grognier, Paulet, and the German, Boehm.[30] These men have received little attention. In addition, there were others who were quietly at work on parasites, infusoria, and fermentation: Agostino Bassi, C. G. Ehrenberg, Cagniard de la Tour, Theodor Schwann, and J. L. Schönlein. Their work was destined to lead directly to that of Pasteur and Koch and to modern bacteriology. Thus the groundwork was being laid for the experimentation that would eventually prove or disprove the hypotheses of the theorists.

Two textbooks of European origin which presented the animalcular hypothesis were used in America. The first was Charles J. B. Williams' *Principles of Medicine,* the American edition of which appeared in Philadelphia in 1844. On etiology, Williams wrote:

> Are infectious diseases the results of invasions and operations of living parasites disturbing in sundry ways the functions and structures of the body, each after its own kind, until the vital powers either fail, or succeed in expelling the invading

[29] Jacob Henle, *On Miasmata and Contagia* (1840), trans. George Rosen. *Bull. Inst. Hist. Med.* 4 (Oct. 1938) 911-83.

[30] La Roche, *Pneumonia and Autumnal Fevers* 318-19.

tribes from the system? Such an opinion has been many times proposed, and is, in a degree, implied in the term *incubation* (sitting on eggs to hatch them), commonly applied to the period between the reception of the infection and the first appearance of the symptoms . . . The microscope should solve this problem, by detecting the germs and growths of these infecting agents, if such exist. Until this be accomplished, the nature of contagion must remain a matter of speculation.[31]

Williams agreed with Holland and Henle that "epidemic diseases are caused by animalcule tribes." Any American student using this text must at least have been made aware that there were in existence other good theories besides the common ones involving miasmata.

The second textbook, *The Pathological Anatomy of the Human Body,* was written by Julius Vogel and translated from the German by George E. Day for English readers in 1847. Vogel paid considerable attention to parasitic animals and plants, such as infusoria, as causes of disease, particularly skin infections, and "putrid decomposition of the elements of the body to a greater or less extent where they appear." [32] He noted the work of his contemporaries, Bassi, Ehrenberg, Klencke, Valentin, Donné and Ricord, but avoided generalizations from their work, similar to those deduced by Henle.

A rather interesting affair took place in England in 1849, concerning the contagiousness of cholera. A quotation in the *Southern Quarterly Review* reads:

Dr. Brittain, of Bristol, appears to have been a diligent observer of the phenomena which usually accompany this disease (cholera). He found in the fluids ejected by cholera patients *organic cells* which, upon being placed under the microscope, exhibit characters such as naturalists attribute to plants belonging to the order of fungi. He also succeeded in finding these cells in the fluids of cesspools and drains, as well as in the atmosphere of rooms where persons have died of, or were labouring under, cholera. Putting these facts together, Dr. Brittain comes to the conclusion that it is the existence of these cells in the body that produced cholera. These observations had been previously advocated by scientific men, and at the last meeting of the British Association at Birmingham, Dr. Alison, a very high medical authority, expressed the conviction that its propagation depended upon this cause.[33]

In a footnote to this statement, the reviewer added more information, remarking that since the above had been written, the Cholera Committee

[31] Williams, *Principles* Ch. I, 85, 79.

[32] Vogel, *The Pathological Anatomy of the Human Body* (London 1847) 441.

[33] R., *South. Quar. Rev.* n.s. 1 (Apr. 1850) Art. VII, 156.

of the College of Physicians of London investigated this fungoid origin of cholera and came to the conclusion that

> the bodies found by Messers. Brittain and Swayne are *not* the cause of cholera, and have no exclusive connection with that disease. In other words, that the whole theory of disease which has recently been expounded is erroneous, so far as it is based on the existence of the bodies in question.[33]

The reviewer righteously added that this was a case of hasty inference from uncertain data. One wonders if Brittain and Swayne ever attempted to push their idea further by means of experimental verification, in spite of the weight of authority against them. Probably they did not.

Littell, in 1866, did not believe in the common miasma theory, calling it a "baseless assumption," yet he admitted it was

> a doctrine which has become thoroughly incorporated with our literature, and forms a part of daily medical thought and expression. In books, in lectures, in conversation—everywhere—it is the organized language of physicians; and those who should have the temerity to intimate a doubt of the truth of a tenet thus universally admitted, would incur no small risk of having their own sanity questioned in turn.[34]

This situation was especially true in the United States.

In the nineteenth century American science suffered from a cultural lag behind Europe. For generations the best physicians were trained in European medical schools, and were under the influence of European teachers and ideas. Yet when they came home very few applied their new training to research work. Nevertheless, towards the middle of the century it became apparent that some original work was being done. Beaumont, Sims, and the discoverers of anesthesia were all speedily and readily recognized overseas. America also had her counterparts in etiological theory, some all the more notable because they do not seem to have been in close touch with European ideas.

III

The earliest American proponent of the animalcular hypothesis appears to have been John Crawford of Baltimore, who published his theories in a local literary journal, *The Observer and Repertory,* in 1807. His work does not seem to have received much attention from his con-

[34] S. Littell, *Relations which Electricity Sustains to the Causes of Disease* (Philadelphia 1866) 4.

temporaries. Crawford's views were essentially that epidemics were caused by an animate contagion. He preferred the animalcular hypothesis to all others. In attempting to prove his theories he drew analogies from nature, noting especially the parasitic ichneumon fly, which lays eggs (parasites) in the body of its intended victim. From this example and many others, he came to the conclusion that "plague, yellow fever, and every other fever and every other disease we experience must be occasioned by eggs insinuated, without our knowledge, into our bodies."[35] Thus Crawford was the first and one of the most definite of the American animalculists.

In the new series of the *Medical Repository,* there appeared a translation of Lancisi's book on marsh miasma, made by Dr. Samuel Latham Mitchill of New York. The chapters dealing with Lancisi's theories on insect transmission and animalculae were printed in 1818. Though his animalcular hypothesis was generally ignored, nevertheless it was there to be read, mixed in with other ideas. An interested physician could easily have abstracted the significant portions and made use of them. Lancisi had noticed that "moschettos" flourished in marshes and stagnant water, and he suggested that they brought infections of lethal particles of animate matter from the marshes to human beings, injecting the poisonous fluid in with their bites. Furthermore, he believed that extremely minute insects and worms were bred in the marshes and that the eggs of these creatures were either swallowed or let into open wounds. He noted that drinking water was full of this "animalcular spawn" and food became loaded with it because insects were attracted to it. Both were swallowed. Lancisi probably hit upon the transmission of malaria when he said that mosquitoes let in the "insect" eggs with their saliva when biting. If he could have proved experimentally that the bite of a mosquito was actually letting in the spores of a parasite, he would have solved the malaria puzzle long before Laveran and Ross. It is remarkable how close to the truth he came, merely by using simple observation and thought.[36] It was a long time before any other writer in either Europe or America developed as clear an idea of the etiology of malaria.

Surprisingly enough the views of Lancisi were very often misinterpreted as favoring the miasma hypothesis. An example of this is found in Knapp's essay on the medical topography of Illinois (1856). Said he:

[35] Significant parts of Crawford's theory may be found in Futcher, *op. cit.* (see note 21) 552. Only one copy of Crawford's work is extant.

[36] *Medical Repository* n.s., 4, Appendix, 201-12, 322-32, 442-67.

> The hypothesis of different kinds of malaria, however, has never come to be generally acquiesced in, but rather the hypothesis assumed is, that obscure and diverse manifestations of disease, or masquerades of maladies, are ascribable to marsh malaria *modified* by place and circumstances. For the last century or more, this sweeping hypothesis of Lancisi has held domain over the records of medical philosophy, growing with its growth and strengthening with its strength; and upon it, as a foundation, are now erected the two superstructural hypotheses of the animalcular and the cryptogamous theories of the *essential* nature of malaria or miasmata![37]

Other writers went even farther than Knapp in misinterpreting Lancisi, and one doubts that they had ever read his work.

The animalcular hypothesis seems to have been seriously considered by Samuel Jackson of Philadelphia. In his volume on the "Yellow or Malignant" fever, published in 1821, he pointed out that Dr. Benjamin Rush Rhees, of the City Hospital of Philadelphia, had studied the black vomit of yellow fever victims with a solar microscope, and had found "innumerable quantities of animalculae" in it. These animalculae, which were probably blood corpuscles, were observed to be active if the victim was alive, passive if he was dead. "Comparative examinations were made of the discharges from the stomachs of patients, ill with autumnal bilious and remittent fevers, but no similar observations were discovered." If the observations had been significant, here would have been proof of the ***disunity*** of fevers. Jackson came to the conclusion that "these very curious observations require to be further and more extensively prosecuted and diversified in order to ascertain what relation those animalculae possess with regard to disease, either as cause or effect."[38]

An experimental approach to this problem was made by someone in Philadelphia between 1821 and 1828. The anonymous reviewer in the *American Quarterly Review* remarked:

> Dr. S. Jackson of Philadelphia, was so kind as to procure for us some decayed vegetables and water, from a miasmatous bank of the Schuylkill. On examination the next morning, by solar microscope, no animalculae were discoverable. No wonder, for to produce the disease in question the birds must have flown. They exist *in the air;* too small for the human sight, unless with powerful apparatus.

A similar experiment was tried in the 1840's by Dr. Joseph Leidy, who examined the waters of the Schuylkill and the Susquehanna in the hopes of finding animalculae. He, too was unsuccessful.[39]

[37] Knapp, *On Nursing Sore Mouth Affection* 226-27.

[38] Samuel Jackson, *Account of Yellow Fever in Philadelphia in 1820* 81.

[39] *Amer. Quart. Rev.* 4. 306; Joseph Leidy, *Flora and Fauna within Living Animals* (Washington 1853) 14-15.

One of the clearest American animalcular theories was expressed by the anonymous reviewer in the *American Quarterly Review,* who was commenting in 1828 on John Macculloch's *Essay on Malaria.* The beginning of the review was innocuous, and the unsuspecting reader was taken through what appeared to be a very favorable review of Macculloch's theory that malaria was a poisonous gas. The reviewer even remarked that the animalcular hypothesis was an ancient one, "at present not considered worth investigation." After two-thirds of the review had been devoted to Macculloch, the author suddenly announced that he was not satisfied with the gaseous theory and developed thirty-two arguments to show that the animalcular hypothesis suited the conditions of malaria better than any other theory.[40] The reviewer used the term *insects* for his animalcules and described them as follows:

> Insects are of all sizes, from the largest to the myriads of various kinds, which nothing but the most powerful microscope can exhibit to our sight. But large or small the laws of their production are the same. The circumstances that will produce a gnat, will produce an insect something less; and so on till we come to animalculae infusoriae, and those that float on the atmosphere. Animalculae and miasmae are connected then: always, in time, place and circumstance. What possible mode of connexion can the reader suggest or assign, but their sameness?
>
> Animalculae do exist in water, in the air, in the food we take in, in our bodies. Sometimes without ill effect. In disease, generally, of which they form a part. Tumours and abcesses usually contain animalcules. . . . It is probable that they exist in the yellow fever: the physicians of Philadelphia are aware of Dr. Patterson's experiment at Bush Hill, in 1820, where the ejections of black vomit exhibited, by the microscope, a congeries of animalculae. The experiment, we hope, will be repeated when it can be. Where the parasite animal is stronger than the life of the animal preyed upon, disease is produced, and the latter falls a victim. Hence the tendency to breed insects in weakly children, especially young females.
>
> Does any man doubt that we inhale animalculae when we breathe: that we eat them and drink them? That when our constitution is vigorous, we destroy and assimilate them, and that when their constitution is more vigorous than ours, they live upon us at our expense? It this not the case with all the vermicular disorders? Is it not likely to be the case, when a new species of animalculae are suddenly generated in full vigor of existence, and when the circumstances that give energy *to them,* decrease our own? This is the case, not only with parasitic animals, such as intestinal worms, but with the moss, the mistletoe, the ivy, and other parasitic plants, which seize upon the trees debilitated by old age, and live upon them.[41]

[40] *Amer. Quart. Rev.* 4. 287, 304-306. This anonymous reviewer left few clues to his identity. He was evidently a Philadelphian, and his mention of the solar microscope leads one to the conclusion that either there was one microscope in Philadelphia which many borrowed, or else this was the Dr. Rhees mentioned by Dr. Samuel Jackson. It might even have been Jackson himself.

[41] *Ibid.* 303-304.

The reviewer acknowledged that he had read the work of Crawford, but denied that he had been influenced by it. Though he believed in the parasitic origin of disease, he did not consider insect transmission, and evidently had not had contact with the ideas of Lancisi.

This reviewer's work influenced Usher Parsons of Brown University to a considerable extent. Though Parsons did not believe in the animalcular theory himself, he did consider it worth mentioning.[42]

Out in the Middle West, far from the chief centers of medical practice, another voice was lifted in favor of the animalcular hypothesis. As early as 1832, Daniel Drake announced his championship of this theory in a book on epidemic cholera. He thought that the disease was caused by minute insects, which, like mosquitoes, deposited their eggs in damp places and stagnant water. The microscopic work of Derham (1727) and Baker (1764) confirmed his belief that there were insects in solid and fluid bodies as well as in the air. The minute ones, he thought, might have an extremely poisonous bite, even worse than that of the mosquito. As another possibility, he reasoned that the eggs of these minute insects floated in the air, like spores of plants, and were inhaled or swallowed by human beings. They then adhered to the mucous membranes or were absorbed into the blood, and the insects developed in the body, producing the disease. Later Drake formalized his work and included it in the famous *Diseases of the Interior Valley of North America,* developing fourteen logical arguments to prove that this was the most satisfactory theory. Of his earlier work, he said:

> Attempt was made to show, that the mode in which that disease [cholera] spreads was more fully explained by the *animalcular* hypothesis than any other. . . . The brief investigation then given to the subject, reinspired my respect for the opinion long before expressed, that autumnal fever, and many other forms of the disease, might be of animalcular origin; and the discoveries since made by the Ehrenberg school, have seemed to render that doctrine still more probable.[43]

Drake's theory is similar to that of the anonymous reviewer, though carried somewhat further. Drake indicated that these "insects" which caused disease had the power of reproducing inside the body, and by this means he accounted for the period of delay or incubation between exposure to disease and its subsequent development. Neither Drake nor the

[42] Parsons, *On the Comparative Influence of Vegetable Decomposition* 11.

[43] Drake, *A Practical Treatise on the History, Prevention, and Treatment of Epidemic Cholera* (Cincinnati 1832) 25-50, 47, *A Systematic Treatise, Historical, Etiological and Practical of the Principal Diseases of the Interior Valley of North America* (Cincinnati 1850) II, Pt. I, 723-27.

anonymous reviewer went far enough to consider their insect a parasite which might be conveyed to humans by means of a larger insect.

Elisha Bartlett, who wrote on the fevers of the United States, thought the animalcular hypothesis was the most reasonable of all the ones offered, but he was careful to point out that it was still pure conjecture.[44]

Two textbooks of the late 'forties contained the animalcular hypothesis. Both were written by professors in the University of Pennsylvania Medical School. The first was *A Treatise on the Practice of Medicine* by George B. Wood, who informs us that "the microscope has laid open an otherwise invisible organic world, in which all the changes with which we are familiar in the visible may take place, beyond the cognizance of the senses, and lead to aerial contamination inconsistent with health."[45] Wood first mentioned the possibility that there might be more than one hypothesis of the animalcular type, writing:

> Two theories have been advanced . . . though further proof is required before either can be received. According to one of them, a peculiar product is generated . . . which is capable of acting as a ferment when it finds the proper materials to act upon, and of reproducing itself or a substance identical with it out of these materials, as yeast is generated. . . . The other theory supposes the cause to be a living, organized, microscopic being, either animal or vegetable, which, produced out of pre-existing germs, under favorable circumstances, is capable of propagating itself indefinitely when these circumstances exist. . . . In favor of the theory which ascribes the disease [yellow fever] to organic germs, is the fact, that it is endemic or original only in a comparatively small portion of those regions of the world, where all the exterior circumstances appear qually to favor its production.[45]

Wood's book went through at least five editions, so there is a good possibility that many students read of the animalcular hypothesis, even though they may not have accepted it.

The second textbook was Alfred Stillé's *Elements of General Pathology,* published in 1848. He noted that experiments had been undertaken to examine the contents of the air: "By condensing the vapours arising in certain malarious districts, small portions of mucus, containing animal matter, ammonia, and hydrochlorate and carbonate of soda have been obtained. This discovery would appear to countenance the hypothesis of Varro, that marshy emanations contain innumerable and invisible in-

[44] Elisha Bartlett, *The History, Diagnosis and Treatment of Typhoid and of Typhus Fever; with an Essay on the Diagnosis of Bilious Remittent and of Yellow Fever* (Philadelphia 1842), *The History, Diagnosis, and Treatment of the Fevers of the United States* (Philadelphia 1847) 347-48.

[45] Wood, *Treatise on Practice of Medicine* 146-47, 306.

sects."[46] Stillé remarked that this idea had been revived by Dr. Henry Holland, but he was rather cautious about subscribing to it. He seems to have preferred the professionally safe miasma theory, though his intellect told him it was not very satisfactory. An explanation for the period of incubation in disease puzzled him:

> How the virus acts during this period of apparent quiescence, and how it finally determines the reaction of the constitution, is altogether unknown. . . . Liebig imagines this matter to consist of a sort of ferment, which, like the minute quantity of yeast that leavens a large lump of dough, gradually modifies the chemical relations of the fluid elements of the body. Other observers, upon the ground of an alleged discovery, that leaven acts by propagating germs, supposes the different sorts of virus to contain animal ova, or vegetable germs, which by rapid generation, fill the body with parasitic insects or invisible plants, whose presence constitutes the disease. When the microscope shall have revealed the existence of either of these sorts of bodies, in the fluids through which inoculable diseases propagate their kind, it will be time enough to give the hypothesis in question a serious consideration.[47]

Thus Stillé rejected the obvious explanation for lack of experimental verification. At one point he noted that there were changes in the blood of malaria patients, a reflection of the work of Dr. Joseph Jones.[48] The best thing about Stillé's etiology was that it at least presented the animalcular hypothesis to his readers as a possible explanation. This was more than could be expected from most medical writers of the period.

The American medical observer of this period who came closest to Lancisi was Josiah Clark Nott of Mobile. Nott, writing of yellow fever in the *New Orleans Medical and Surgical Journal* in March 1848, ridiculed the marsh miasma theory. He observed that:

> It would certainly be as philosophical (as the malarial theory) to suppose that some insect, or animalcule, hatched in the lowlands, like the mosquito, after passing its metamorphoses, takes flight, and either from preference for a different atmosphere, or impelled by one of those extra-ordinary instincts, which many are known to possess, wings its way to the hilltop to fulfill its appointed destiny.[49]

Nott credited the greater danger of night air to the presence in it of insects, which are rendered inactive by light and heat during the daytime, and emerge at night. In this category he mentioned moths, "musquitoes,"

[46] Stillé, *Elements of General Pathology* 98.
[47] *Ibid.* 104-105.
[48] *Ibid.* 99-100.
[49] Nott, "Yellow Fever Contrasted with Bilious Fever—Reasons for Believing it a Disease sui generis—Its Mode of Propagation—Remote Cause—Probable Insect or Animalcular Origin —&c.," *New Orleans Med. Surg. Jour.* (March 1848) 580, 588, 595.

aphides, and others. The insect hypothesis again would account for the "fact" that trees were a barrier to malaria, because the insects could use them for a resting place. Nott sarcastically added, "It should be borne in mind, too, that the very writers who . . . run their malaria up trees, are those who tell us that its specific gravity is so great that it lies on the ground! !" He also explained to his readers that heat destroyed "the germ of the disease." Nott carried his theories one step further than other animalculists, for, noting the reproductive powers of infusoria, he stated: "With these few facts before us, how much more easily may we account for the spread of yellow fever from a focus, by the insect, than by the Malarial hypothesis—here is something tangible and comprehensible."[49] He knew that plants had parasites, and vaguely inferred that a similar situation might hold for humans, but on this point his theory lacks clarity.

One of the most striking animalcular theories was that of John K. Mitchell of Philadelphia, who declared in 1849 that diseases were caused by fungi. In his own words:

> The only theoretic view of malaria to which I incline, is that which refers marsh fevers, and some of the epidemic diseases, to a living organic cause, capable of reproduction by germs, as is alleged of contagious diseases; but unlike that latter in this, that the germs are not reproduced in the organism of the sick, but exteriorly to, and independently of, the human body. In other words, that as the germs of contagious diseases are reproduced *in* the body, the germs productive of malarious and other non-contagious diseases are elaborated and re-elaborated *out of* the body and independently of its agency. One is the product of *person*, the other of *place*. This notion is sustained by the fact that organic azotized substances are the only things detected in marsh air or dew, which can possibly affect the health injuriously.[50]

Mitchell noticed that fungi flourished in wet places and so did fevers; fungi spread at night and fevers were caught at night; fungi spread by means of spores, and disease could be better explained by this means than any other. He ascribed most diseases, particularly fevers, to the action of fungi on different parts of the anatomy.

Mitchell's theory had considerable influence both among his colleagues in Philadelphia, and among his contemporaries, particularly in the west. John Evans, writing in the *Northwestern Medical and Surgical Journal* in 1849, mentioned the many theories as to the origin of cholera, including the "cryptogamous." Daniel Drake read and approved Mitchell's book. It was reviewed by R. (probably Richards, the editor) in the

[50] J. K. Mitchell, *Cryptogamous Origin of Fevers* 31-32, 40 ff.

Southern Quarterly Review in 1850. The reviewer was an expert mycologist and he checked all Mitchell's statements against the latest work in mycology, to the complete vindication of Mitchell. The poor microscopes of the day made it difficult to check hypothesis experimentally, but the medical scientists were always in hopes of getting better equipment which would reveal more about the invisible world. E. S. Gaillard, who held to an ozone theory himself, mentioned Mitchell in passing. Francis Peyre Porcher, in 1861, spoke of Mitchell's theory. Joseph Leidy was opposed to the theory, regarding it as unproven and of little value. He himself had swallowed quantities of "monas, vibrio, volvox, vorticella, etc." without any ill effects. In addition he had searched for animalculae spores in the waters, dews and air of malarious localities, but had found nothing.[51]

Mitchell had introduced his theory in a series of lectures in 1846-47, but had refrained from publishing them because he was afraid that publication would lead to controversy. He also feared he lacked conclusive proof. However, in 1848, a similar theory was put out in England by Charles Cowdell, and Mitchell's friends urged him to publish his own and claim priority. With the exception of Cowdell, Mitchell does not mention any others who also had fungus theories although the idea had been published in different form by Ludwig Boehm in 1838. Mitchell also claimed that the cryptogamous theory had "wide dissemination on this side of the Atlantic . . .," though he failed to elaborate this statement.[52]

Several lesser known men adhered to the animalcular hypothesis. One was Alexander H. Stevens of New York City who suggested that cholera was propagated from person to person by means of seeds of fungus or animalculae.[53] M. L. Knapp, in an essay on "Etiological Deductions—Malaria," reflected:

> It is analogically evident that the assumed miasmata must be animalculæ, for the heat and slime of the bottoms force into existence nolens volens the products of decay, which like old cheese quicken with a sort of putrefactive vitality, and can surely be smelt if not seen, and easily be imagined to take wing by day and go to roost at

[51] Evans, "Observations on the Spread of Asiatic Cholera and Its Communicable Nature," *N. W. Med. Surg. Jour.* (Chicago 1849) 4, 21-22; Drake, *Diseases of the Interior Valley*, II, Pt. I, 272 n; *South. Quart. Rev.* 1. 146 ff.; Gaillard, *Essay on Intermittent and Bilious Remittent Fevers with their Pathological Relation to Ozone* (Charleston 1856) 7-56; Leidy, *Flora and Fauna* 14-15.

[52] Mitchell, *Cryptogamous Origin* iii-v; Cowdell, *Disquisition on Pestilential Cholera;* Ludwig Boehm, *Die Kranke Schleimhaut . . . in der Asiatischen Cholera* (Berlin 1838).

[53] Alexander H. Stevens, "On the Communicability of Asiatic Cholera," *Trans. Med. Soc. State N. Y.*, 174 (1850) 34.

night, seeking the interior of cabins in order to keep worm and dry like other well-bred fowls and insects. Furthermore the musquitoes, gnats, flies, devils-needles, toads, turtles, and turkey-buzzards are abundant as the insect swarms of an offended God sent upon the hosts under the hard-hearted Pharoah, therefore the miasmata must be of animal character or nature—a sort of animal stream or invisible quintessence of *suspected* animal life, well enough proved by its odor and poisonous effects, the fevers, as also by its being killed by the frosts, though never yet isolated or seen by mortal eye aided by microscope![54]

This is a somewhat different point of view from that of the average animalculist. Knapp thought it possible that the invisible life could be either animal or vegetable in nature. In other words, he would subscribe to either animal or cryptogamous theories. He also had his own theory to the effect that, similar to scurvy, malarious diseases were due to deficiency of fresh fruits.

In 1857, a commission in Norfolk, Virginia, submitted a report on the origin of yellow fever, in which the various members came to the following conclusion:

In the present state of science, it is not possible to determine what is the material cause of yellow fever, in as much as it is not tangible, and cannot be subjected to examination and analysis. It is only by a careful process of induction from all known facts in relation to the disease, that we can hope to arrive at a correct theory of its origin and propagation. The hypothesis, which best explains most of the known phenomena of the disease, is that its material cause is some organic matter endued with the property of rapid reproduction, either in the soil or atmosphere congenial to it, but not capable of being reproduced in the human body [non-contagious]. That this matter, whether of animalcular or vegetable character, is a production of tropical regions, and is only spread in temperate climates when introduced into them by ships.[55]

A dissenting opinion was voiced by A. B. Williman, who was a contagionist in regard to yellow fever. He said, "Again, I think that the cause of yellow fever is some minute material germ, capable of reproducing itself when given off from the human body suffering under this disease."[56] It may be noted that both the majority and dissenting opinions believed in some sort of animalcular hypothesis, but differed as to the method of transmission involved, thus continuing the old contagion con-

[54] Knapp, *Inquiry into Nursing Sore Mouth Affection* Ch. III, 251-52.

[55] *Report of the Origin of the Yellow Fever in Norfolk* (Richmond, Va. 1857) 43.

[56] *Ibid.* 44.

troversy. This controversy could not be settled until the advent of bacteriology produced a solid foundation of fact with which to work.

An unusual book appeared in Charleston in 1861. It was Francis Peyre Porcher's *Illustrations of Disease with the Microscope*. The pictures show what Porcher found in his microscopic analysis of the blood and other body fluids, but none of the cells drawn appear to have been microorganisms. Probably his microscope was not powerful enough to reveal them. In his text, Porcher stated that "scarlet fever, measles, small-pox, are each *sui generis,* are disseminated by exposure to special morbid poisons, working in the blood in the nature of a ferment, and which are peculiar in their nature."[57] The next step was to discover the "special morbid poisons."

After the epidemic of cholera in Chicago in 1866, there was considerable speculation as to the cause of the disease. N. S. Davis considered the infection due to "seeds" or "germs" scattered through the city, and, though he believed the atmosphere to be an important factor in the dissemination of the infection, he also noted that the "prevalence of sporadic cases with other bowel affections" as a possible factor.[58] American cholera writers of this period were still unaware of the work of John Snow showing cholera to be a water-borne disease.

The general run of physicians were not animalculists. In 1828, J. W. Huestis called attention to the animalcular hypothesis in order to oppose it. Said he, "There are others again who, with still greater appearance of sagacity, pretend that the bilious or yellow fever is produced by countless myriads of animalcules, secretly introduced into the human system, and thence exciting it into morbid action." Some writers, like O. B. Knode, admitted frankly that they had no idea of the nature of the malarial poison. R. S. Holmes admitted that he had heard of various theories of the animalcular type, but he added that "to mention [them] . . . to persons who have really seen many locations of miasmata, would be but to abuse their patience." Nevertheless, he added, "there are some medical gentlemen, however, who are even yet skeptical on the subject of miasmata." William H. Tingley claimed that "most etiologists endeavor to explain the origin of disease by introducing some unknown external agent,—as

[57] Porcher, *Illustrations of Disease with the Microscope* (Charleston 1861), 79.

[58] Davis, "How Far do the Facts Accompanying the Prevalence of Epidemic Cholera in Chicago, during the Summer and Autumn of 1866, Throw Light on the Etiology of that Disease?" Presented to the American Medical Society, May, 1867.

miasm, malaria, fungus, decaying animal or vegetable matter—instead of searching for more rational causes." In this case the "more rational causes" were climatic ones. La Roche disbelieved the animalcular hypothesis because the experiments of his friend, Joseph Leidy, were negative on the subject. As late as 1874, William Norcom of North Carolina still held the hypothesis of bad night air and he advised his patients to avoid it.[59]

Joseph Jones of Savannah had some acquaintance with the work of Ehrenberg on infusoria. He used the microscope in connection with autopsies and with it he analyzed the blood *quantitatively.* His most important observation was that the malarial poison destroyed red blood corpuscles. In regard to treatment he was against the use of blood-letting and purging because he thought that malaria was depressing to the system and not an inflammation.[60] Porcher had read Jones' work, and though he had no idea of the existence of microbes or of their function, he made a careful examination of the blood for pus, bile, sugar, chemical reactions, acidity, and types of corpuscles. His illustrations show that his microscope was not a very high-powered one. Porcher had also been influenced by Stillé's views on miasmata in the blood. He quoted Stillé to the effect that "many diseases . . . arise from *a retention in the blood not of substances actually foreign to it, but of some which have assumed a new form,* rendering their presence in the system incompatible with health."[61] Porcher did not find any of these substances.

In spite of all opposition, the animalcular theory continued to be preferred by a scattering of men to whom no other explanation was satisfactory. Even these men failed to attempt to prove or disprove their ideas by experimentation. Medical quarrels at this time were still settled by the academic means of sporadic observation and logic—a last remnant of the old scholastic thinking. Mechanical aids to scientific work were poor. Surprisingly little use was made of the microscope. Even when the microscope was used, the doctors had little understanding of what they saw.

[59] J. W. Heustis, "Remarks on the Endemic Disease of Alabama," *Amer. Jour. Med. Sci.,* 2 (1828) 32; O. B. Knode, "A Report on Malarious Fever," *St. Louis Med. Surg. Jour.* 14, No. 5 (Sept. 1856) 410; Holmes, "Malaria and Topography," *St. Louis Med. Surg. Jour.,* 5, No. 6 (May 1848) 520; Tingley, "On the Action of Climatic Causes in the Production of Disease," *St. Louis Med. Surg. Jour.* 14, No. 5 (Sept 1856) 386; La Roche, *Pneumonia and Autumnal Fever* 318-19; Willam A. B. Norcom, *Haemorrhagic Malarial Fever* (Raleigh, N. C. 1874) 15-16.

[60] Joseph Jones, "Observations on Malarial Fever," *South. Med. Surg. Jour.* (June 1858) 5, 12, 178.

[61] Porcher, *Illustrations of Disease* 28; Stillé, *Materia Medica and Therapeutics* 2. 641.

The influence of the animalcular school of thought was not very widespread and, to judge from the decreasing volume of literature, appears to have almost disappeared by the late 1850's.

IV

One final consideration connected with the causation of disease is the nature of the agent of transmission. The contagion controversy concerned itself with the question as to whether man or some external agent was responsible. Holland and Evans suggested the agency of man in cholera, and Holmes and Semmelweis held the same thesis for puerperal fever. None of these views was generally accepted. Holmes in later years enjoyed reminding the medical profession of the soundness of his early opinion. "I think I shrieked my warning louder and longer than any of them," he declared, "and I am pleased to remember that I took my ground on existing evidence before the little army of microbes was marched up to support my position."[62] At least one contagionist lived to see himself vindicated.

The most prevalent theory of agency, of course, was that of the air and its contents. Closely confined air was thought particularly virulent. Water was a lesser agent and then only putrid water was considered, not water that *looked* pure. Some observers, such as Lancisi, went so far as to note that air contained insects, which in turn might be the cause of disease or the agent of transmission. The term "insect," as used in the sense of cause, was synonymous with microbe. It is unusual to find it used in the sense of agent, though a *specific type of insect* was suggested as such by Lancisi.

Richard Mead used "insect" to mean "microbe" and he believed that the eggs of his insects were transported from place to place, causing the disease when they hatched.[63] Daniel Drake also thought that insects were the actual cause of disease, but he expressed it differently:

> We assume the existence of malaria, of mineral exhalations, of meteoration, of contagion; let us, in like manner, assume the existence of poisonous, invisible, aerial insects, of the same or similar habits with the gnat; let us assume still farther . . . that these have instincts which may direct their migrations . . . and that they are liable to be carried by high winds into elevated and dry places, which they would not fre-

[62] O. W. Holmes, Letter to the American Gynecology Society in Philadelphia, 1893, printed in William Osler, *An Alabama Student and Other Biographical Essays* (Oxford 1929) 66.

[63] *Discourse Concerning Contagion* 16.

quent from choice; finally, let us suppose that some cause has augmented the number of individuals of some species, and we shall then have all the theory which is necessary to explain most of the facts connected with our Epidemic [cholera].[64]

In his later works on diseases, Drake made his insect analogous to "the smallest invisible gnat," a species of sand fly whose bite inflames the skin. From this he deduced that animalculae when introduced through the skin might also have a poisonous effect.[65] The insects of the anonymous reviewer in the *American Quarterly Review* were also of this type.

Insects were mentioned by many writers, but their function as carriers of disease was understood by none except Lancisi. James Lind noted that hordes of insects were to be found in the tropics. Robley Dunglison knew that mosquitoes were generated in stagnant water and so did William Currie. Noah Webster, Benjamin Rush, and James Copland realized that swarms of insects often appeared just before an epidemic. A general connection between the swarms of insects and disease was made by some correspondents of Webster, and by Parsons and Copland. A correspondent of Professor Dunglison, in a letter printed in the *Medical Examiner,* mentioned mosquitoes in Polynesia, as well as miasmata, without any malaria, and suggested that the cause of the disease "must be sought for in sources other than marshy exhalations." Edward Miller came very close to the theory of insect transmission as held by Lancisi when he supposed that miasmatic poisons might be "injected into the system by some venomous animal." His vagueness in the sentences elaborating the subject make it seem rather unlikely that he had any clear idea of the connection between insects and disease. Drake, as noted above, had a much clearer conception, even though it was not the actual causation in the disease with which he was concerned.[66]

V

At the time when great advances were being made in Europe and the new science of bacteriology was being developed, American etiology

[64] Drake, *Practical Treatise on Cholera* 44.

[65] Drake, *Diseases of the Interior Valley* 2. 724.

[66] James Lind, *An Essay on Diseases Incidental to Europeans in Hot Climates* (4th ed., London 1788) 68; Dunglison, *Human Health* (new ed., Philadelphia 1844), Sect. III, 58-94; William Currie, *An Historical Account of the Climates and Diseases of the United States of America* (Philadelphia 1892) 57; Webster, *Brief History of Epidemics* 2. 16-17; Rush, *Medical Inquiries* 4. 175; Copland, *Dictionary* II, "Epidemics," 777; Webster, *Collection of Papers on the Subject of Bilious Fevers* 3, 76; Parsons, *Comparative Influence of Vegetable Decomposition* 17-18; Copland, *Dictionary,* II "Disease," 569; *Medical Examiner* 7 (1843) 269; Edward Miller, *Works* 47.

was still in the same position it had been when the century opened. The animalcular theory had not been accepted and, as Littell points out, it was virtually forgotten or mentioned only for ridicule. The interests of the 'forties, which led to scientific experimentation and eventually to the germ theory of disease in Europe, came to a dead end in America; and when the new ideas were imported, they were treated as entirely novel, and their American forerunners usually ignored. The micro-organism theory of disease, which was so generally discredited during the first seventy years of the century, was accepted in the short period between 1875 and 1890. In this acceptance also, America lagged behind Europe, not really convinced until the germ theory appeared in Hugo W. von Ziemssen's *Cyclopedia of the Practice of Medicine,* published in English translation in 1874-1878, and more thoroughly in George M. Sternberg's translation of M. Magnin's *Bacteria* in 1880.[67]

Why was America so slow in adopting scientific medicine? And above all, why did American doctors fail to develop the science of bacteriology from etiology and pathology when they had the same background and the same raw materials with which to work as did the Europeans? Some of the answers to these questions are apparent.

In the early nineteenth century America lacked both public and private support for pure science. There was no patronage tradition. No organized research was carried on in the United States, only sporadic attempts, as the classic case of William Beaumont. There was little prestige to be gained in research because the commercial spirit of the Anglo-Saxon world placed the emphasis on practice—reflecting the business man's sense of values. Furthermore, the lack of financial support for research forced doctors into practice, regardless of their personal choices. Osler's

[67] See Joseph C. Richardson, *Phila. Med. Times,* July 10, 1875; Austin Flint, "Medical and Sanitary Progress," *Harper's Mag.* (June 1876) 53, No. 313, 70-84; John T. Carpenter, "The Local Origin of Constitutional Diseases," *Trans. Med. Soc. State Penna.,* (1878) 12, Pt. 1, 140; W. F. Muhlenberg, "Address in Hygiene: Germs," *Trans. Med. Soc. St. Pa.* 14 (1882) 82-101; H. F. Formad, "The Bacillus Tuberculosis, and some Anatomical Points which Suggest the Refutation of its Etiology Relation with Tuberculosis," *Proc. Phila. Co. Med. Soc.* 5 (1882-83) 29-41, "Discussion," 41-42. By this time the profession was generally hostile to the NON-acceptance of the germ theory of disease; John M. Keating, "The Presence of the Micrococcus in the Blood of Malignant Measles," *Trans. Coll. Phys. 1883,* 115-33; James T. Whittaker, "The Bacillus Tuberculosis," *Trans. Coll. Phys. Phila. 1883,* 149-62. Whittaker refers to the old *contagium vivum* idea; Irving A. Watson, "Water Pollution—Wells," *Trans. New Hampshire Med. Soc. 1883,* 93-108; Ezra M. Hunt, "The Origin of Disease and Micro-Organisms as Related Thereto," *Trans. Med. Soc. N. J. 1888,* 95-111; E. M. Hunt, "Notes on Progressive Studies in Etiology, and Especially as to Micro-organisms as Sole Factors of Disease," *The Medical News,* Philadelphia, 52, No. 11 (March 17, 1888) 281-85.

"Alabama Student" was a typical example. Practice gave the physician his professional reputation. Even the university professors were chosen from practicing physicians, and a man who lacked a large and lucrative practice would have found it hard to attract students. The emphasis on the practical in American outlook demanded utility at a time when the doctors could promise the least. The public was not interested in pure science, represented by etiology and the identification of diseases, but only in applied science, represented by treatment. Since the profession had no other source of income, it generally found it expedient to follow the public demand. This held back the development of medicine because treatment could not be successful, except by chance, when the causes of disease, or even the nature and identity of the diseases themselves were unknown. In this case emphasis on applied science over pure science was putting the cart before the horse. Finally, there was a deterioration of personnel in the medical profession itself. Sects, such as homeopathy and others more esoteric, discredited the profession as a whole. Medical education was at a very low level, with dozens of small, worthless colleges turning out students who had attended a course of lectures, but had never had any clinical instruction, and often had never seen or treated a patient. Most of the best doctors were educated abroad, at Edinburgh and Paris.[68]

Poor instrumental aids, legal hindrances, and lack of facilities also impeded American medical development. Microscopes were rare and not of good quality. In some states it was against the law to hold autopsies, and everywhere dissections were seldom held. Hospitals were scarce and often poorly managed. In contrast to the European tradition, no experiments were tried out on charity patients.[69]

In addition to the financial, philosophical, educational, material, and legal impediments to medical research, there was one more great barrier which would have prevented progress even if all the other factors had been favorable. This was the mental barrier. The profession opposed innovation. It was satisfied with itself. As no great mental effort was required to acquire its body of knowledge, and prestige-satisfying additions could be made to that body from an armchair, there was little incentive to

[68] Richard H. Shryock, *Medical Research in the United States: Past and Present.* To be published by the Commonwealth Fund for the Committee on Medicine and the Changing Order of the New York Academy of Medicine. Ch. II, "The Gallic Era: 1820-1860."

[69] *N. Y. Jour. Med.* 4 (March 1845) 159; Samuel Jackson, *The Principles of Medicine* (Philadelphia 1832), pref., xvi-xvii.

adopt the rigorous techniques of scientific method. The lethargic scholasticism of earlier days remained with American medicine and made argument rather than experiment the foundation of truth. The treatment accorded the animalcular hypothesis illustrates this spirit. By argument, American doctors were able to dispose of the theory, completely and finally. It was so dead by 1875 that the discoveries of Pasteur and Koch came as an entirely new idea. Abroad, the interest in etiology had led to the development of the science of bacteriology. At home, unable to make this transition for herself, America had to follow the lead of Europe across this frontier.

Henry Sigerist–The Man and His Stature

GREGORY ZILBOORG

WHEN NEWS CAME that Dr. Henry E. Sigerist had resigned as the William H. Welch Professor of the History of Medicine at Johns Hopkins University, his friends, his acquaintances, his students, the press, and the scientific world in general were startled—startled and distressed. The first reaction was one of profound regret, but when the motives behind Dr. Sigerist's decision became more clear the sense of impending loss yielded to admiration for his wisdom and courage. For he has decided to abandon his professorshop, to withdraw from the time-consuming demands entailed in administering a university department, to leave his friends in America and the many pupils to whom he has always been devoted, in order to turn his entire attention to writing a sociological history of medicine. Through years of research he has accumulated a mass of data, but his teaching and lecturing, his contributions to his own and other journals, and his active part in the affairs of various learned societies have left him no opportunity to bring the material together into a comprehensive whole.

To those who have followed Sigerist's career and his intellectual and spiritual evolution, this step has not come as a complete surprise. During the anxious years of World War II he felt keenly the lowering of standards of research in the history of medicine and in higher education in general. In 1943 at the ceremonies commemorating the four-hundredth anniversary of the *Fabrica* of Vesalius he reproachfully warned that it was wrong to consider the history of medicine "non-essential" in time of war. To those who advanced this premise, he addressed himself with impatient scorn. "They document by their attitude," he said, "that culture to them is not a necessity like air and water, but merely a superficial ornament, good

Editor's Note: This article was distributed in an abridged form on May 9, 1947 at the dinner honoring Dr. Henry E. Sigerist before his departure for Switzerland. Together with the article by Dr. Milton I. Roemer that follows, it is representative of the many tributes paid to Dr. Sigerist.

for normal times but one that may be discarded lightheartedly in a period of emergency, that is, when we need it most desperately."

In this same speech, later published in his book *The University at the Crossroads*—he threw out other thought-provoking ideas. Why, he asked, is it legitimate to play bridge in time of war, and not legitimate or not proper to commemorate the four-hundredth anniversary of Vesalius' *Fabrica?*

> I really fail to see why we should not come together on solemn occasions to commemorate an event of the past, to acknowledge a debt, to renew our faith in science, in mankind, in the ideals of our predecessors that are still our ideals today. What good will it do us to win a military victory if we are starved culturally, if our imagination and enthusiasm have been so stifled, that we no longer have the courage to stand up and build the world of tomorrow?

These words summarize Sigerist's faith and his depth of conviction.

And very shortly afterwards he ponders the university's dilemma and utters the warning which is as general as it is personal, and as much a threat as it is a melancholy reproach:

> If the university is to remain an active center of research imparting first-hand knowledge to students; if a university appointment is to remain attractive to researchers, we must think along new lines and must devise a different system. It must be one that will give the researcher time and leisure to engage in his studies, not only late at night when he is close to exhaustion. It must permit him to give few but significant courses on subjects that happen to be close to his heart. It must give him time to meet with his colleagues and students, leisurely, not with an eye on the clock. . . . We may have to think of a [university] department not in terms of a hierarchy but of a cooperative, highly specialized group in which the main researchers will have the least administrative burden. . . . I do not know what the best solution is but I know that we must find one, and that we must find it soon.

Henry Sigerist's solution of his own problem is rooted in a deep personal faith in what Goethe states simply as: "Die Geschichte der Wissenschaft ist die Wissenschaft selbst" ("The history of a science is the science itself.") In his book on *Medicine and Human Welfare* Sigerist's belief is set forth in his own words: "The historian of medicine wants to know what has happened in the past and what is happening today. He endeavors to understand the phenomena of health and disease and their significance for the individual and society. In doing this, in not

merely accepting the facts as they present themselves but in reflecting about them and trying to interpret them, he imperceptibly enters the field of philosophy."

In order to prepare the ground for the fruition of these ideas, one must inspire youth with a love of the classics in medicine, kindle in them the fire of faith, and inculcate the need for cultural endeavor. This Henry Sigerist has done with unmatched success for over twenty-five years—in Zurich, in Leipzig, in Moscow and Toronto and Johannesburg, in London, in Paris, in Delhi, in Honolulu, and in widely separated centers in the United States. He has followed the example of Paracelsus and traveled far, covering enormous distances, talking to great numbers of people of all ages, races, and conditions. He has not interrogated them with selfish curiosity, has not eyed them in the manner of a scientific voyeur collecting facts, but rather he has tried to understand their needs, their wishes, their hopes—all this as a partner in the struggle for knowledge and for the betterment of man's lot on this earth.

It is this warm human sympathy and deep understanding that has won for Henry Sigerist so great a number of friends, admirers, and followers. It is given to few to be able to maintain such serene simplicity of relationship, always encouragingly understanding and always understandingly encouraging. These qualities have made him a most persuasive teacher, for out of his sensitive insight has come the rare ability to relate the history of medicine and medical culture with the problems of the present. Seldom has it been given to a man to live so vitally in the present without losing the perspective of the past.

But Sigerist has not accomplished this happy synthesis without effort and in his introduction to his translation of the Maurice Arthus' preface to *De l'Anaphylaxie à l'Immunité* he offers guidance to those who would follow his example:

> Our young people who are at the beginning of their scientific career, who have taken the heroic resolution to devote their life to research will, I am sure, find inspiration and encouragement in the words of Arthus. His honesty, his fanatic devotion to science, his insistence upon originality and last but not least his method, still the most promising one today, will provide them with guidance and will renew their faith in the dark hours of discouragement through which every researcher must go.

One feels that Sigerist himself has had first-hand knowledge of these "dark hours."

This experience has deepened his perception of the economic, social, and racial obstacles which stand in the way of many a capable student. Yet Sigerist has never become melancholy. On the contrary, his broad sympathies have brought him in contact with so many and such a variety of people that he has developed a clear and realistic perspective. In this he is a true scientist and a true historian, possessing tolerance, understanding, and responsiveness in a rare degree. His versatility has made him at home and happy and enthusiastic with people of varied interests—with an amateur photographer in the darkroom, with a sedate university professor, with the difficult student who wanted to understand a simple idea of Aretaeus which appeared to him both obscure and foolish but which he dared not confess to the professor, with the mine or automobile worker whose philosophy of life is always under pressure of economic stress, with the cook who is learning from him how to baste a bird with wine, while he in turn is learning how to prepare an American dressing for a Thanksgiving turkey. For in addition to an academic cap and gown, Sigerist wears with pride and joy the laboratory apron of a photographer and the kitchen chef's cap and coat. In his many files you may find, written in his own hand, extracts from Arabic manuscripts, new formulæ for fine-grain photographic developers, and the best recipes for Russian Borshtsh or German Hasenpfeffer. In his address books you may find the names and addresses of leading scientists, of laboratories, hospitals, libraries, restaurants all over the world, or little grocery shops which carry a special brand of tea. Human life and living in all their manifestations are to Sigerist the quintessence of his faith and ambition.

One might wonder whether one is not crossing the boundaries of privacy in thus writing about Henry E. Sigerist. After all, Maupassant was right when, in refusing information about his personal life to a newspaperman, he said: "To the people belong our works and not our faces." True, but Sigerist's interests influence his personality to an unusual degree and thus become a part of his work, his books, his lectures. The universality of his tastes and sympathies is a part of what he is as a teacher and research worker and thinker. He tries to understand and feel for everyone who is living and feeling—"from the dustman, down to the duke," with this exception: he will join the dustman and help him with

his chores, but he would not join the duke at the horse races. For Sigerist's social conscience is intense and all pervasive because it is based on the deepest sense of tolerance. He tolerates even stupidity and harshness. I know of no one who so well combines tolerance, without condoning or condemnation, and social scorn, without petulant hate.

This is the reason one has never heard him say an unkind or harsh word about Karl Sudhoff, his mentor and master, when the latter toward the end of his life joined the Nazi Party. Sigerist tried to understand this—he did not rage about it. He glanced at the formal announcement of Sudhoff's funeral sent out by the Nazis at Leipzig and merely shrugged his shoulders as if to say, "Is it not sad?"

He is well-nigh the only one among us who can speak openly, forcefully, yet inoffensively to the dyed-in-the-wool reactionary against reaction or to the unjust against injustice. He is well-nigh the only one who with courage and fire and conviction and sobriety has told our universities how foolish it is to be afraid of earnest and detailed studies of the Soviet Union. He is among the few who have stood before the medical profession and told them how unseemly it is to oppose government health insurance and other movements embodying a socialized form of medical care and research.

In this respect Sigerist is the true incarnation of freedom, and of academic freedom in particular—he lives it. It is to the everlasting credit of American universities that a man of Sigerist's liberal faith has been permitted to come to his full intellectual stature during the difficult years first of depression and then of war.

Those who would look for faults and inconsistencies in Sigerist's social philosophy and sympathies would always do well to remember that his enthusiasms are intimately interwoven with and rooted in serious thought, that they are not temperamental enthusiasms, and that Sigerist is not a careerist. He is a man of free study and free thought. This is why he could translate these words of Arthus' with so much love and conviction:

> And thus it happens that the two major qualities, the spirit of independence and the spirit of originality are suppressed and sterilized for a long time and without fail by the enthusiasts and by the careerists, for different reasons but with equal success. And this is why the scientific work of our young people today is often so

dull, so colorless, and so sad. It lacks the characteristics that are those of robust health and a free life, namely spontaneity and unexpectedness.

I shall not waste words about the careerists. But to the others in whose hearts the sacred torch is lit, I cry as loudly as I can: young men, preserve jealously your independence and your originality! Honor your teacher, as is fitting, and respect him sincerely. But accept his directions only if they seem justified and only in so far as they respect your independence and your liberty. Follow his example wittingly only, that is only if it seems to you good, noble and worthy. If you discuss scientific matters with your teacher, ask questions, argue. Do not accept his words, all his words, without reservation and criticism. Criticize with moderation, of course, and first of all respectfully, but criticize firmly.

It is remarkable that this expression, "criticize firmly," could almost be taken as Sigerist's motto. He applies it to his own scientific thought as much as to that of others. It is this realistic self-criticism that finally led him to the step of resigning his post at Johns Hopkins. It is not that he found himself in a state of hopelessness, in a state of flight from the American scene. This American scene he knows well and loves very much. It is here that his scientific thought grew to unusual depth. It is here that he formulated his medical and social philosophy. It is here that he "grounded himself" in the English language, so much so that now whenever he has to give a lecture in French he finds it necessary to make more detailed advance notes than had been his wont. When he came to this country, Sigerist did not belong to those who are inclined to say that they know something before they have learned it well. He spoke with neat scorn about those European travelers who emphasize

over and over again the surface aspects of American life—skyscrapers, chain-stores, jazz and the movies. If [he continued], after a few weeks of ill-tempered traveling, you write a book condemning the country, you have a good chance of being rewarded with an Academy prize. I have been in Rome, in Greece, in most European countries, and have had a glimpse of the Orient. From all my travels I brought home strong impressions, but this journey across the American continent was the most stirring experience I had ever had. Never before had I felt so clearly that I was envisaging history in the making.

These words are from Sigerist's *American Medicine,* written in German after his first visit to America sixteen years ago and then translated into English. The American scene was a revelation to Sigerist.

I was tremendously impressed by the United States [he wrote in his later book, *The University at the Crossroads*]. I found democratic institutions dear to me and

found the country utterly different from what it is usually pictured by European travellers who see only the façade and the material aspect of American life. I was particularly impressed by the universities, by their earnest desire to raise standards of scholarship and to carry on the torch of learning. I found them fully aware of their great responsibility at a time when European institutions were breaking down under political and financial pressure. Great interest was shown all over the country in medical history, the importance of which had been emphasized by all medical leaders. And I found urgent social problems of medicine, eagerly discussed by all groups involved. The Committee on the Costs of Medical Care was at the peak of its activities. What appealed to me particularly was the dynamism of America and its brave experimental spirit, so utterly different from the stifling self-satisfaction and fossilizing feeling of superiority so often met with in Europe.

And now we shall do Sigerist as well as ourselves justice if we do not yield to that rather selfish feeling of loss at the thought that he is leaving this country and that he abandons his academic work—for none of this is really true. The village in the Italian part of Switzerland to which Sigerist moves his study is closer to New York now than was Chicago sixteen years ago when he first touched these shores. Sigerist is going to the country to write his history of medicine. The last comprehensive history of medicine, that of Sprengel, is a little over a hundred and twenty-five years old. In this respect, as in so many others, Sigerist is determined to fill the gap, to make up for the cultural lag. No one is so well equipped to do it today. To accomplish this personal and historical mission, Sigerist wisely decided to restrict the immense number and variety of his activities. Those who are his pupils, followers, friends, and admirers should feel that Sigerist ought to be congratulated on his decision, for he reached a point in his scientific activities when he so reasonably recalled Arthus' admonition to "hold to the principle of unity remembering the good old proverb: *qui trop embrasse mal étreint.*"

Sigerist will enter on his work in the Swiss village with that serenity, that sense of freedom and perseverance with which he has always taught his students, and which he so well taught himself. He is going to write in English and the Oxford University Press in the United States will be his publishers. He does not leave us, he does not leave the United States. He does not leave or abandon anything or anyone. Perhaps the words which Arthus so ardently addressed to his students, and which Sigerist so well rendered into English, could be fittingly addressed by Sigerist

to his students and followers and friends, and could also be humbly redirected to Sigerist with all the respect and admiration he deserves:

> And if in order to preserve your independence, your human dignity, you experience sometimes troubles, difficulties and all that follow, preserve your independence in spite of all and preserve it fiercely. The day will come when you will congratulate yourself for having done so. Of course, you will not be granted official honors, social prestige and all the vanities which you would have bought at the price of thought. Be not affected nor depressed by it. The day will come when you will be infinitely happy about it.

A Tribute to Henry Ernest Sigerist

MILTON I. ROEMER

DOCTOR SIGERIST is going to leave the United States. When the news came, it was a personal blow to thousands. What could we do to show how we felt about it? At least we could pay him a tribute and try to say what he has meant to us in America for these fifteen years. This is a message of farewell to Henry Ernest Sigerist from those young physicians of America who have looked to him as a teacher and a friend and a source of inspiration.

We learned about him first before he came here to live. This doctor from Germany had come over in 1931 to lecture at some American universities on the history of medicine. While he was here, he made a study of the whole fabric of American medicine and when he returned to Leipzig he wrote a book about it. It was translated from German into English and, when we read it, it was as though we had just discovered the country in which we live. This was not a collection of biographies of men of medical science and how they had made their technical contributions. This was a story of America, its past and present; the figures in the foreground happened to be doctors and nurses and there were hospitals and clinics in the scene—but the rich background was our whole national life. This was a different kind of medical history. Here were portrayals of the past that lived and had meaning today. Here were analyses that gave us an understanding of the complex problems in the distributions of medical care to the people, about which we read on the front page of the morning newspaper.

By the time *American Medicine* appeared, Doctor Sigerist was back in this country. He was appointed Director of the Institute of the History of Medicine at John Hopkins University. Doctor Welch had brought him over to carry on. A fanatic named Adolf Hitler was beginning to make headway in Germany, only about a dozen years after her defeat in the "War to End Wars." Doctor Sigerist saw trouble ahead. Germany was no land in which to explore the lessons of the past with a free and liberal spirit. The Old World was collapsing but America was still fresh with opportunity.

How quickly he mastered our language! How incisively he told his story. I will never forget the first time I heard him talk. It was before a group of medical students. In five minutes the audience was captivated. This friendly man with a sparkle that leaped from his black eyes, talking with broad gestures of his hands, his arms, his shoulders, his head, his whole figure—with his strange accent, half German, half French—gave us a sudden breath of Europe and of the World. His words were always simple and, when an idiom of the Continent was here and there detectable, it was more charming than foreign. He said things that were perhaps shocking to some of his older medical listeners, facts and judgments that were critical of the status quo. But the Great Depression had taught even the smuggest that America was not the best of all possible worlds and the presentation was so rich and so convincing, there was no answer.

Again we heard him talk, and again and again. It was not always before medical students. There were meetings of organizations in the field of foreign affairs, there were consumers' groups, societies for American-Russian friendship, labor organizations, general university audiences —audiences of intellectuals and audiences of the common people. Always he had something new to say, something of special significance for this audience. And each time he spoke he made hundreds of new, devoted friends.

Doctor Sigerist did much more than give us knowledge. He became our friend, our advisor. When medical students from many different schools decided to organize an association to promote their common welfare and encourage interest in the social aspects of medicine, Doctor Sigerist was our godfather. When internes set about to build a national body, we went to Doctor Sigerist for guidance. He assisted us in establishing journals for these bodies, he helped us to plan our conventions, he gave us strength against the storm of opposition that greeted a socially-minded movement among young people.

It was not long before the American Association of the History of Medicine took on new life under the leadership of Doctor Sigerist. The *Bulletin* which had been launched as an organ of the Johns Hopkins Institute was broadened to become an organ of the entire Association. Its circulation was greatly extended. Active local chapters of the Association were stimulated throughout the whole country. Doctor Sigerist encouraged membership and participation particularly by young physicians

and students. Medical history became more than a pastime for the mellow years of professional retirement.

While Doctor Sigerist had made America his home, he did not stay bound to it. There were new and highly significant happenings in other parts of the world. Along with American medicine, Russian medicine, he felt, had most to teach. And so in the summer of 1935 he went to the Soviet Union to study at first hand this strange nation with its "five-year plans." When he returned he told us here of the remarkable organization of public health and medical care in the socialist sixth of the earth. He returned again in 1936 and again in 1938 leading a group of American physicians and others through this vast country. In 1937, we were given *Socialized Medicine in the Soviet Union*. Here we found for the first time what "socialized medicine" really was.

Tours and books were not enough to convey to American physicians and others the high importance that Doctor Sigerist saw in Soviet medical organization and medical science. A formal organization for the interchange of information was needed. The American-Soviet Medical Society was organized in 1943 to answer this need. Under Doctor Sigerist's leadership and inspiration it grew rapidly. Again young people were made to feel an integral part of this new and pioneering group.

Meanwhile Doctor Sigerist taught and studied and wrote. Active concern with the vital world around him did not divorce him from the never fully explored sources of the past. Medieval Latin texts were translated and analyzed, and the whole human and social character of medicine in the Middle Ages and Renaissance was intensively investigated. Paracelsus, Vesalius, Boerhaave, and others were subjected to new scrutiny and evaluation. Always Doctor Sigerist insisted on the importance of consulting original sources. A series of medical classics was undertaken in 1937 and we were impressed with their truth and their beauty. In 1938, Graduate Weeks in Medical History were initiated at the Institute and from all over the nation came physicians and others for learning and inspiration. In Central and South America, in Europe, everywhere, Doctor Sigerist's influence was felt, not only through the work of the Institute, but through his own active participation in international associations devoted to the history of science and of medicine.

What grand experiences Doctor Sigerist would provide in introducing us to certain great figures of the past who spoke medicine's social aspirations of today. There were Johann Peter Frank, von Pettenkofer, Ramaz-

zini, and others. And even the masters of America's own recent past, like Welch or Osler, acquired new meaning through the interpretive accounts of Doctor Sigerist. The brief and sweeping histories of occupational diseases, medical licensure, gynecology, hospitals, the physician's status, and a dozen other subjects—talks usually presented at a meeting of some professional society—gave us a breadth of understanding that others had not conveyed in volumes.

And all this was done with such magnificent simplicity. We who had recently completed or were still engaged in some eight years or so of college and medical school, had had more than our fill of ostentatious erudition and pedantry. How delightful and refreshing to hear or read wisdom in words of a single syllable. What a pleasure to find quips and good humor amid the knowledge. The stories about tea and wine and water closets and truffles and the spelling of his own Latin-Italian-German-Swiss name gave us the flavor of a modern Chaucer or Voltaire.

The demands on Doctor Sigerist increased at a geometric rate. With each new written contribution, with each new meeting he addressed, there came a dozen requests for new writings and new addresses and new committees to serve on. He could not possibly accept them all, though we wondered how he had the strength to accept as many as he did. Yale University asked him for a series of lectures on the Terry Foundation and all of us who could not hear them read them in *Medicine and Human Welfare* in 1941. Cornell invited him to give the Messenger Lectures and in 1943 we had the thrill of reading them in *Civilization and Disease*.

Nearly every summer he continued to travel. In 1939, it was South Africa. To the University of Witwatersrand we young physicians owe a special debt of gratitude, for it gave Doctor Sigerist the occasion to sketch his own educational background. We learned about his own early education, the men who had influenced him, his struggle to find himself, and how he finally came to devote himself to that aspect of humanism which is medical history. In the summer of 1940, travel abroad was out of the question for Europe was in flames, but America was not yet at war. So Doctor Sigerist went through the United States to observe various new plans of medical care organization. He took us with him through the medium of full reports of his observations in the *Newspaper PM*.

One of the newer health programs, that of a labor union in Detroit, asked Doctor Sigerist to serve on its advisory committee. Many a gilt-edge organization had received polite refusals to similar invitations, but

this was an organization of industrial workers out to blaze new trails in health service planning. He accepted. Among the credentials of Henry Ernest Sigerist, following the M.D., the LL.D., the Litt.D., the citation of William H. Welch Professor of the History of Medicine at Johns Hopkins University, and the offices held in a dozen learned societies, one finds that he is a member of the Medical Advisory Committee of the Health Institute of the United Automobile Workers, C.I.O.

The war changed many things. For a long time Doctor Sigerist had been planning his major opus—a comprehensive history and sociology of medicine. He was now so filled with the subject, he thought at last he could begin. But the menace of world Fascism stopped that. The librarian of the Welch Medical Library went into the armed services and Doctor Sigerist was named to carry on. It was an administrative job making tremendous demands on an already over-burdened schedule. He was appointed a technical adviser to the Foreign Economic Administration of the federal government and made weekly trips to Washington. Even a regime of working past midnight regularly was not adequate for the production of a work of this scope. It was all Doctor Sigerist could do to keep the torch of social science in medicine burning at the Institute during the dark years of the war. During this period, he wrote some essays on the failures of our civilization which—in the midst of our zeal for military victory—gave us perspective on the character of a social and economic system that led to two world-wide slaughters in a generation. Future generations of doctors will look back on the program for a new medical school that Doctor Sigerist outlined in 1941—surprised that such a stir should have been created by a proposal to train physicians as social servants, as well as technologists. We are grateful that these essays were brought together in 1946 in a volume, *The University at the Crossroads.*

So unselfish a life was bound to produce severe physical strain. In 1941 and again in 1943, we were distressed to learn that Doctor Sigerist was taken ill. But even his illnesses were made to yield permanent values. For the visit to Saratoga Springs, New York, where he went for treatment and rehabilitation, inspired him to look into the history of American spas. Out of this grew a number of papers which gave us a new appreciation of physical medicine, with all its organic as well as psychological subtleties.

After recovery there was no letdown. In 1944, the newly elected

Cooperative Commonwealth Federation government of Saskatchewan, Canada invited Doctor Sigerist to make a health survey of the province and recommend a program. The final report won immediate attention in the field of planning for organized rural medical services and gave us guidance on comparable problems in this country. In the same year, the government of India selected two Americans as members of a committee to make a health survey of their land. Doctor Sigerist was one, Doctor Joseph W. Mountin the other. And so some of us learned about the vast health problems of the greatest colonial outpost of the British Empire.

Those among us who were fortunate enough to be enrolled as students at Johns Hopkins University saw Doctor Sigerist as the professor. If we were at the medical school, we heard the lectures of a master on the history of medicine—the story of medicine developing in an ever-changing, complex world of social, economic, political, cultural struggle and progress. Here was the world's most accomplished medical historian talking with us. If we were at the School of Public Health and Hygiene, or just within traveling distance of Baltimore, we attended his evening seminars on the sociology of medicine. What an experience, these seminars! Everything was discussed from housing and health to patent medicines to methods of paying physicians to Soviet medical education to agricultural health programs to Japanese health insurance. The devotion toward the teacher was of an order of magnitude that only the disciples could appreciate. The pity was that more of us could not come to these seminars.

If Doctor Sigerist won countless friends, he made some enemies. There were bound to be difficulties for one so outspoken in his evaluation of social forces. They were pressing days in the winter of 1940-1941, when Doctor Sigerist defended the actions of Soviet Russia against Finland. Whether or not one agrees with his views, one must agree that this took a kind of courage which even the later revelation of Mannerheim's regime as the northern outpost of Nazism did not adequately reward. No one who read the opening chapter of his book on Soviet medicine could mistake Doctor Sigerist's basic point of view. It was evident in his magnificent little paper on "Science and Democracy" read before the American Association for the Advancement of Science in 1937. He declared it in a talk at Columbia University in May 1938 when he out-

lined his plan for a history of world medicine. It was clear in everything he wrote.

Despite his unorthodox philosophy, the remarkable thing was that this fearless man won such freedom for himself. There was no denying his stature, and when the societies of learned and conservative men wanted a brilliant and original presentation on any aspect of the history of medicine, they had to look to the doctor in Baltimore.

When Doctor Sigerist leaves us, perhaps the most poignant memories will be of the hours he relaxed with us. It was never really relaxation, because every moment was exciting, like the pleasure of a long walk in the snow. There were the "bull sessions" in the lobby or hotel room during the evenings of a busy convention; there were the snacks in a restaurant after an absorbing lecture. And when we visited Doctor Sigerist at his home, what rich and mellow hours! The library, the incredible scrap books of photographs and clippings (How did he have time for all this!), the fragrant Chinese tea brewed before us—never to be forgotten.

How many of us came to Doctor Sigerist for personal advice about our careers in medicine, I do not know. For each of us he found time, and to each of us he gave his full attention. We always went away satisfied, more firmly assured in our plans that we had been before. I am not sure, but I suspect the reason was that Doctor Sigerist tried first to fathom what we really wanted to do ourselves and, when he had, he would then assure us enthusiastically that this was just the thing to do. Perhaps he helped us to see, in our own minds, what we really wanted. In any case, he always helped us.

Doctor Sigerist belongs to all in America who want to see a better world—who want to see the benefits of science brought to all people everywhere. But I think most of all he belongs to the young men and women with a sparkle in their eyes reflecting the dawn of tomorrow. He belongs most to the young because he is young and will always remain so. He will belong to the young of every generation.

Now Doctor Sigerist returns to the land of his youth, Switzerland. It is right that he should go, because his great *History* must be written. Nothing he can do is of more importance; yet the demands on him here are too great. He made a beginning in the fifteen years on American soil, one volume. There are seven more to be written. All the world awaits this book.

Are there other factors contributing to his departure? Doctor Sigerist came to America to escape reaction in the Old World. He now leaves an America in which he finds increasing signs of the same forces. But this is no escape. It is a reflection of our failure, the failure of our culture to provide a setting where men of Doctor Sigerist's stature may work and create, free of routine administrative burdens. Would another social milieu conquer this problem? Whatever the reasons, Doctor Sigerist will always be with us, for what he has left in America is here to stay. The young men and women, charged with his spirit, will work to make American medicine the social service that he taught us it must become.

We wish you, Doctor Sigerist, a farewell from the bottoms of our ten thousand hearts. We hope it is really an Au Revoir.

Some Recent European Publications Dealing With Paracelsus

GEORGE ROSEN

PARACELSUS died in 1541, having lived through a transitional period of struggle when Europe was sloughing off its medieval skin and emerging in its modern form. Four hundred years later, the anniversary of the death of Paracelsus was celebrated in the midst of a terrible, world-wide conflict, which again marked the end of one era and the beginning of another. Even under these unfavorable conditions the occasion was noted with appropriate ceremonies in the United States and abroad; this was particularly the case in Switzerland. Subsequently a number of publications dealing with various aspects of the life and thought of Paracelsus appeared in several European countries. With the end of the war, this Paracelsus literature began to appear in the United States. The purpose of this review is to survey a number of these publications, and to assess the contribution that they have made to our knowledge of Paracelsus.

The high-point of the Swiss celebrations was the Paracelsus Congress at Einsiedeln (October 4-6, 1941). The stimulus provided by this meeting led to the founding, one year later, of the Swiss Paracelsus Society (*Schweizerische Paracelsus Gesellschaft*). Its purpose, the Society announced, would be to study the activities of Paracelsus, to establish the influence exerted by these activities in the diverse fields of knowledge—medicine, natural science and philosophy—and to present these studies in suitable publications. In particular, the annual volume which the Society decided to publish as its organ was designed to serve this purpose. Three volumes of the Yearbook of the Swiss Paracelsus Society (*Jahrbuch der Schweizerischen Paracelsus Gesellschaft*) for 1944, 1945, and 1946 have appeared so far. Before dealing with their contents, however, it will be of interest to consider several biographies and anthologies intended as introductions to the life and thought of Paracelsus.

In 1942, Ildefons Betschart published a biographical study of Paracelsus.[1] This volume contains all that is actually known or surmised about the great Swiss doctor. In this small book the author has succeeded in

[1] Ildefons Betschart; *Theophrastus Paracelsus. Der Mensch an der Zeitenwende*, Verlagsantalt Benziger & Co., Einsiedeln und Köln 1942. 171 pp. Illustrated.

presenting the meteoric career of Paracelsus in an attractive and exciting manner. Betschart is able to do this because of the sympathy and psychological insight with which he approaches his subject. This biography is intended for the reader who knows nothing about Paracelsus, and needs a guide who will introduce him to the man from Einsiedeln and his views. Betschart's book adds nothing to our knowledge, but it can be recommended to all who read German as an excellent introduction to the life and thought of Paracelsus. Something on this order should be available in English.

Very different in purpose and approach is the biographical sketch prepared by Basilio de Telepnef.[2] Written by a member of the Swiss Paracelsus Society, this essay is to be the first step in a campaign to introduce "the true Paracelsus" to the English reading public. Accordingly, this small book is published in English. While the employment of a stilted and unidiomatic English is not necessarily a serious obstacle, yet the American or British reader will undoubtedly look askance at the author's peculiarities of style and idiom.

More fundamental, however, is that the reader soon finds this volume to be nothing but a propaganda tract intended to serve the purposes of a Paracelsus cult. The author shows not the least pretense to objectivity, and exhibits an attitude of uncritical adulation in the treatment of his subject. Apparently the author and his circle believe that Paracelsus anticipated almost every important discovery of modern science, and reverently study every obscure Paracelsian remark to find such nuggets. For example, Dr. J. Strebel, in a short introduction, refers to Paracelsus as having "clearly recognized" the vitamins. In a similar vein, de Telepnef speaks of him as "a forerunner of the Swiss Red Cross-doctors" (p.27), and as "the actual originator of experimental research" (p.62). Furthermore, he says, "It is amazing how near the grandiose vistas of Paracelsus's world-conception in these two books [the first two books of the *Opus Paramirum*], which are still awaiting the physicists' detailed investigation and comprehension, approach to the present light-theory of a man of de Broglie's outstanding knowledge. . . . Another modern illustration of Paracelsus's great idea is the perpetual inter-action and transformation of the trio: Photon (neutral entity), Electron (negative), and Positron

[2] Basilio de Telepnef. *Paracelsus, A Genius amidst a Troubled World. A Short Essay on the Life and the Main Works of this Great Physician, Scientist and Philosopher.* Zollikofer & Co., St. Gallen, 1945. 93 pp. and 1 map.

. . ." (p. 64). Again we learn that in the *Astronomia magna* "the processes of hypnotism, suggestion, and psycho-analysis are stated with great force and clarity" (p. 74). Finally, in an addendum, de Telepnef quotes several passages from *Das Buch Meteororum* implying that these have some prophetic connection with the atom bomb.

Such uncritical Paracelsus worship is hardly likely to arouse in the American or English reader a desire to become further acquainted with the career and ideas of Paracelsus. Trashy productions of this kind will only add to the erroneous views about Paracelsus. It is therefore not too much to ask that any future publications issued abroad for the English reading public be inspired by another outlook and spirit.

The third biographical production to be considered here differs from the preceding two volumes.[3] It is an anthology of passages from the works of Paracelsus in which he presents autobiographical data. Collected and edited by Karl Bittel, this anthology offers practically an autobiography of Paracelsus. The volume is divided into the following ten parts: Childhood and youth; Student Days and *Wanderjahre*; In Salzburg during Reformation and Peasant War (1524-25); Along the Upper Rhine (1526-28); In Nürnberg (1529-30); In Switzerland (1531-35); The "Great Surgery" Appears (1536-37); In Carinthia (1537-40); Death in Salzburg (September 24, 1541); Paracelsian Views on Life. These ten sections contain 65 passages. The text is based for the most part on Sudhoff's edition of the *Sämtliche Werke,* although a few passages stem from other sources. Each selection is documented to show the source, and the sources are discussed in a bibliographical section (pp. 140-146). All in all, this is a very useful compilation for anyone who wants to have the basic facts regarding the life of Paracelsus presented in his own words.

Two anthologies dealing with the thought of Paracelsus appeared in 1942. In a systematic form they present selections of important passages. The anthology edited by Erwin Jaeckle[4] is intended to present the cosmogony of Paracelsus and to show that this body of ideas is a truly unified system. Jaeckle has arranged his selections to show the circularity of the Paracelsian world-view. Beginning with the Great Mystery (*Mysterium magnum*), it proceeds through incarnation in the Creation, loss of original

[3] *Paracelsus. Leben und Lebensweisheit in Selbstzeugnissen,* Ausgewählt und eingeleitet von Karl Bittel (Reclams Universal-Bibliothek Nr. 7567/68) Verlag Philip Reclam Jun., Leipzig 1944. 149 pp. and 1 illustration.

[4] Theophrastus Paracelsus. *Seine Weltschau in Worten des Werkes.* Herausgegeben von Erwin Jaeckle. Atlantis-Verlag, Zürich 1942. 386 pp.

Unity, and returns to the Great Mystery after restoration of the divided Cosmos through medical and religious activity. Jaeckle has provided an introduction wherein the ideas of Paracelsus that form the basis of the volume are subjected to a logical and philosophical analysis. This is a work of considerable merit. While the present writer dissents from Jaeckle's construction, because in his opinion it is too artificial, it does help to bring prominently into focus the fact that underlying Paracelsian medicine is a body of ideas regarding Man, Nature, and the Universe without which the medical views are not entirely comprehensible. One doubts, however, that the Paracelsian ideas were ever as systematized as they are made to appear in this volume.

The second of these anthologies, edited by Jolan Jacobi,[5] a disciple of the psychologist C. G. Jung, is a run of the mill collection. The editor intentionally omitted all material relating to Paracelsus, the surgeon and the alchemist. Not only has specifically medical material been left out of this book, but also anything that smacked of superstition. The central theme of the volume is the Paracelsian view of the relations to each other of God, the World, and Man. Sudhoff's edition has been used as the source from which the selections have been taken. In order to render the material more accessible to the reader, Jacobi has partially modernized the language of Paracelsus. Toward this end a glossary of Paracelsian terms has also been provided, as well as a biographical sketch of Paracelsus, a bibliography, and separate indexes for the text and the illustrations indicating their origin. Although the volume contains 150 illustrations taken from contemporary sources, their value is reduced by very poor reproduction, and by the fact that the legends explaining them are not placed under each picture but instead are collected at the back of the book. While this volume may have some slight value as an introduction to certain aspects of Paracelsian thought, it does not present a balanced picture and may therefore create a false impression in the reader's mind.

A collection of Paracelsian works which differs from the two anthologies discussed above began to appear in 1944 under the editorship of Dr. J. Strebel of Lucerne.[6] This edition has as its aim the presentation in

[5] Theophrastus Paracelsus. *Lebendiges Erbe.* Eine Auslese aus seinen sämtlichen Schriften mit 150 zeitgenössischen illustrationen. Herausgegeben von Jolan Jacobi. Rascher Verlag, Zürich und Leipzig 1942. 315 pp.

[6] Theophrastus Paracelsus. *Sämtiche Werke in Zeitgemässer kurzer Auswahl.* Herausgegeben von J. Strebel. Bd. I. *Paracelsus der Medizin- und Kulturhistoriker.* Zollikofer & Co., St. Gallen 1944. 329 pp., illustrated. Bd. II. *Der philosophischen Schriften erster Teil: Paracelsus als Metaphysiker, Magier und Mystiker.* Zollikofer & Co., St. Gallen 1945.

modern German of the complete texts of those Paracelsian writings which the editor has selected as most important. The edition is planned to appear in five volumes, of which the first two have already been published. These volumes have been beautifully produced and are a credit to the publisher.

The first volume, which bears the incomprehensible sub-title, "Paracelsus der Medizin- und Kulturhistoriker," contains the three books known as the Carinthian trio (Seven Defences; Labyrinthus medicorum errantium; and the Carinthian Chronicle), and selected letters of Paracelsus (to Erasmus, Christoph Clauser, the students of Zürich, Amerbach, Dr. Magenbuch, the municipal council of Nürnberg, and Wolffgang Thalhauser, the municipal physician of Augsburg). There is a long introductory essay by Strebel which attempts to place Paracelsus in his historical context. Unfortunately, however, it is marred at several points by that same uncritical Paracelsus worship to which attention was directed previously. Strebel unconsciously provides a comic touch in his efforts to reconstruct a picture of Paracelsus's unknown mother by supplying Hirschvogel's well-known Paracelsus portrait with a woman's head kerchief. Despite these gaffes, however, the contents of volume one are well suited as an introduction to Paracelsus, for they give us the great medical revolutionary in his most personal aspect.

An appendix to this volume presents the results of Basilio de Telepnef's effort to reconstruct the routes followed by Paracelsus in his peregrinations from about 1512 to 1524. For this purpose de Telepnef utilized Sudhoff's edition, and in his chronological reconstruction he cites the passages on which his conclusions are based (see pp. 287-302).

Volume II of Strebel's edition deals with Paracelsus the metaphysician, magician, and mystic, and contains works published in volumes XIII and XIV of Sudhoff's edition. These are *Philosophia magna, Volumen primum,* in which Strebel includes the *Liber de animalibus ex sodomia natis* (Sudhoff, XIV), *Philosophiae tractatus quinque,* and *Liber de imaginibus* (Sudhoff, XIII).

The Paracelsus celebrations of 1941 produced a number of interesting interpretations of Paracelsian thought. Probably the most stimulating of these were two essays by C. G. Jung, the well-known Swiss psychologist. These essays, published in book form in 1942 under the title *Paracelsica,* offer an interpretation of Paracelsian philosophy as a personal system of

psychology in terms of alchemical conceptions.[7] This view is based on Jung's analysis of the psychological aspect of alchemy which he interprets as a path of salvation of the soul.[8] As interpreted by Jung in modern terminology, for Paracelsus alchemy is the process of individuation, the process through which the individual becomes perfect and immortal, through which he becomes "man" in the highest sense. The key that unlocks this door is the *opus alchymicum,* the alchemical act, which Jung explains as analogous to the Christian sacrament. For this reason alchemy is so fundamental for the physician and is one of the pillars of medicine. In conclusion, Jung asserts that Paracelsus was a pioneer not only of chemical medicine, but also of empirical psychology and psychological medicine. He admits, however, that Paracelsus may not have been conciously aware of the train of ideas developed in these essays, so that in the end Jung's interpretation must be regarded as an attempt to bring Paracelsian thought into line with Jung's own ideas. At any rate this volume, as well as the one on psychology and alchemy, should be read by every serious student of Paracelsus.

An important study of a different kind appeared in 1943 in Sweden. This was a book by Sten Lindroth on Paracelsism in Sweden till the middle of the seventeenth century.[9] The significance of Lindroth's work is that he undertakes to trace in detail the reception of Paracelsian ideas in Sweden and the reaction that these evoked, not only in medicine but also in general intellectual and religious life.

After reviewing the doctrines of Paracelsus concerning man and nature, Lindroth deals in successive chapters with Paracelsism in Sweden in the sixteenth century, including a discussion of Paracelsism and Ramism (that is, the doctrines of the French humanist Pierre de la Ramée); Johannes Bureus and the influence of Paracelsus on his mystical thought; Paracelsism at Uppsala University; the influence of Paracelsism on the thinking of Sigfridus Aronius Forsius; Paracelsian religion in its relation to spiritualism, the Rosicrucians, and pietism; the new drugs and their introduction into the Swedish pharmacopoeia; Georg Stiernhielm and the Paracelsian elements in his natural philosophy; and the pansophists.

Paracelsism was first introduced into Sweden in the 1580's. Simon

[7] C. G. Jung: *Paracelsica. Zwei Vorlesungen über den Arzt und Philosophen Theophrastus.* Rascher Verlag, Zürich und Leipzig 1942.

[8] C. G. Jung *Psychologie und Alchemie.* Rascher Verlag, Zürich 1944.

[9] Sten Lindroth. *Paracelsismen i Sverige till 1600—talets mitt.* Almqvist & Wiksells, Uppsala 1943.

Berchelt's *Om Pestilentzien* (1589) introduced some of the Paracelsian medical doctrines. The new medicaments came into general use gradually, but it was not until after 1630 that these gained prominence and exerted an important influence on the development of chemical science in Sweden. The new theories of disease, sharply opposed to the Galenic pathology, gained adherents. Johannes Raicus was for a short time (1627-1632) a warm exponent of Paracelsian medicine. His *De phtisi ex tartaro* is written by an adherent well acquainted with the new medical doctrine. Paracelsian theories of nature likewise exerted an influence. Sigfridus Aronius Forsius incorporated important elements in his *Physica* (1611). From 1630 on, the principles of Paracelsian chemistry, in the form given to them by Sennert and his students, were introduced into academic instruction at the University of Uppsala. The religious and utopian aspect of Paracelsian thought also exercised a significant influence on the Swedish pansophists, theosophists, and Rosicrucians, of whom Johannes Bureus is an important representative.

One can give only a sketchy account of the highlights of this important study. For details the reader must resort to the original. But at this point one cannot but deplore that Lindroth wrote his book in Swedish. While it is understandable that he wished to write in his native tongue, the stubborn fact remains that the majority of Paracelsian scholars cannot read Swedish so that the results of this pioneer study are out of reach. Lindroth might have appended to each chapter a summary of its contents in English, French, or German, and this would have been of some assistance to those who do not read Swedish. Perhaps Lindroth will summarize his results in an English article; I am certain that the *Journal of the History of Medicine* would be delighted to publish it.

The significance of Lindroth's book lies in the fact that he has opened a new field for Paracelsian scholars. Heretofore most writers on Paracelsus have either dealt with his career, interpreted and commented on his medical and philosophical ideas, or have edited and translated his works. No effort was made to trace the routes by which his teachings were received in various countries, the reactions evoked by them, the degree to which various elements of Paracelsian thought were incorporated in the thinking of others, the changes undergone by Paracelsian ideas, and the lasting effects of these in medicine, philosophy, religion, and other aspects of human life. In the last analysis, it is only on the basis of such studies that the medical historian will be able to achieve

a more balanced evaluation of Paracelsus and his work. It is greatly to be desired that other studies of this kind be undertaken; and it is with a sense of gratification that we present in this issue of the *Journal* Kocher's study of Paracelsism in Elizabethan England.

The first annual volume of the Swiss Paracelsus Society appeared in 1944 under the title *Nova Acta Paracelsica.*[10] This title has reference to the *Acta Paracelsica,* of which five parts appeared from 1930 to 1931.[11] The *Acta Paracelsica* were issued by the German Paracelsus Society, and included a Paracelsian bibliography of 1089 items compiled by the late Karl Sudhoff. The *Nova Acta* are conceived by those who issue these annual volumes as a continuation in spirit of the earlier publication. For this reason, the first volume features a bibliography of publications since 1932, as well as several items of earlier date that were omitted by Sudhoff. The present one was compiled by Ildefons Betschart, whose Paracelsus biography has been noted above. The items are arranged alphabetically without any annotation. It comprises overwhelmingly articles published in Swiss, German, or Austrian newspapers and periodicals, and on the whole is disappointing. Many of the items listed are of no scientific value whatever. Some are vitiated by the Nazi ideology of the authors (e. g. Conti, Fritsche), others are simply journalistic ephemera written on the occasion of the Paracelsus celebration. The American and English literature is very scantily represented. Betschart attributes the incompleteness of the list to the then prevailing situation created by the war.

In an introduction Betschart reports the various Paracelsus celebrations that were held in Switzerland, and describes the founding of the Swiss Paracelsus Society. In addition, there is an obituary of the French Paracelsist René Allendy (1887-1942).

The papers in this volume are, as might be expected, all devoted to Paracelsus, his activities and ideas. Walter Bigler, in a stimulating paper deals with the fundamental principles that should underlie the study of Paracelsus. He points out that the work of Paracelsus must be studied and interpreted within its own cultural and intellectual context. To do justice to Paracelsus and his ideas, an explanation must attempt to discover the criteria that correspond to his views. In other words, the investigator must attempt to determine the categories in terms of which his work must be

[10] *Nova Acta Paracelsica.* I. *Jahrbuch der Schweizerischen Paracelsus Gesellschaft, 1944,* Verlag Birkhäuser, Basel 1944.

[11] *Acta Paracelsica.* Heft 1-5. Verlag der Paracelsus Gesellschaft, München 1930-1932.

judged.[12] This requires the application not only of biology, but also, and perhaps chiefly, of philosophy and theology. Bigler concludes that only on the basis of such analysis will it be possible to do justice to the attitude of Paracelsus to alchemy, and to test the correctness of Jung's interpretation, which in his opinion (and in the opinion of this reviewer) is highly questionable.

J. Strebel discusses the status and trends of Paracelsian studies in Switzerland. He regards the task of Swiss researchers as twofold: first, the preparation of a complete bibliography of all the original works of Paracelsus to be found in public and private Swiss libraries, and secondly, the publication for a general public of original works by Paracelsus.

In a paper dealing with the childhood of Paracelsus in Einsiedeln, Karl Bittel emphasizes once more how few really authenticated facts we have about the life of the great Swiss physician. With regard to his childhood, Bittel points out that no documentary evidence exists, and that the only data are contained in autobiographical statements by Hohenheim himself.

The ideas of Paracelsus in relation to the currents of thought of his own time and of our day are discussed by Fritz Medicus. He discusses his influence on such thinkers as Böhme, Schelling, and Baader; the rejection of Paracelsus by the mechanical materialists of the eighteenth and nineteenth centuries; and the revival of Paracelsus towards the end of the last century, especially by Karl Sudhoff.

Of the other papers in the first volume of the *Jahrbuch,* only three deserve some comment. Hans Kayser, whose Paracelsus anthology is still one of the best of its kind, presents a brief but stimulating analysis of the contradictions in Paracelsian thought. Pointing out that any attempt to develop an orderly, systematic, and completely integrated system out of the Paracelsian doctrines soon runs into insuperable difficulties, Kayser believes that this may be explained by the type of thought employed by Paracelsus, which he designates as ***Formendenken.*** More specifically, this means thinking in terms of symbolic and visual imagery rather than in terms of purely abstract logical concepts separated from concrete reality. Kayser indicates that the concepts based on visual imagery employed by Paracelsus represent a synthesis between a world of experience derived from intuitive perception and a world of experience based on conceptual

[12] See Hans Leisegang; *Denkformen.* Walter de Gruyter & Co., Berlin and Leipzig 1928.

knowledge, on knowledge involving logical and systematic relationships. The point raised by Kayser is important because it deals with a basic principle, that the historian, before he can venture to interpret a text, must determine the categories of thought employed in it. In a sense, it leads to the point of view of a sociology of knowledge (cf. Scheler, Leisegang, Mannheim).

Donald Brinkmann, in his article "Mythos und Logos im Weltbild des Paracelsus," raises the question whether the modern European is truly able to comprehend the thought of Paracelsus in its various aspects. Pointing out that while Paracelsus has been celebrated as a pioneer of modern medicine, decried as a charlatan and magician, or considered as a typical representative of late Renaissance philosophy, he asks whether these interpretations actually comprehend the innermost nature of Paracelsian thought. Brinkmann feels that Paracelsus is a truly mythical figure, and that consequently his essential nature cannot be adequately comprehended through either rational, logical, or religious concepts. In addition, one should also try to consider the Paracelsus phenomenon as a mythical figure and to analyze it in terms of mythological categories. From this point of view the non-rational elements in the thought and practice of Paracelsus are brought into a proper perspective for an interpretation.

The role of the Swiss physician Dr. Hans Locher in the rehabilitation of Paracelsus is the subject of a paper by J. Strebel. It was in 1851, on the occasion of the 500th anniversary of the entry of Zürich into the Swiss Confederation, that Locher published his memoir in which he described Paracelsus as the Luther of medicine.

Volume II of *Nova Acta Paracelsica*[13] likewise contains a number of interesting contributions. Karl Bittel deals with the Basel period of Paracelsus. He makes it clear that while Hohenheim taught at the university he never was actually a member of the medical faculty. Among the reasons for the enmity that he aroused, Bittel indicates that economic and social factors played a significant role. Hohenheim attacked vested interests of physicians and apothecaries, and eventually these opponents drove him from Basel. Most of this is already to be found in Sudhoff's biography, but Bittel presents the story in greater detail.

Basilio de Telepnef presents the results of his efforts to determine

[13] *Nova Acta Paracelsica. II. Jahrbuch der Schweizerischen Paracelsus Gesellschaft, 1945.* Verlag Birkhäuser, Basel 1945.

the various universities and schools that Paracelsus visited and attended. He also tries to date these visits and to determine how much time Hohenheim spent at each institution. As in his other study of the peregrinations of Paracelsus, Sudhoff's edition is used as a base.

An interesting contribution to the iconography of Paracelsus is Rudolf Henggeler's paper on "Alte und neue Paracelsus-Medaillen."

Erwin Jaeckle undertakes a comparison of Paracelsus and Agrippa von Nettesheim. He comes to the conclusion that while the latter in his thought simply represents elements of the past, Paracelsus created something new, that his synthesis was pregnant with the elements of the future.

Other articles in this volume deal with Paracelsian astrology (Strebel), the psychiatry of Paracelsus (Allendy), Paracelsus in relation to the spirit of modern technology (Brinkmann), and discussions of two Paracelsian works—on nymphs, sylphs, and pygmies, and on the *Liber de fundamento scientiarum sapientiaeque,* both by Strebel.

The volume of *Nova Acta Paracelsica*[14] for 1946 is a definite let-down after the two preceding annuals. Of the thirteen contributions, five are by Basilio de Telepnef, four are by J. Strebel, and four are by other authors. Telepnef comments on the *Paragranum,* traces the wanderings of Paracelsus between 1512 and 1525, to which he adds a chronology, and a list of the university cities visited by Paracelsus, and contributes a memoir on Karl Sudhoff. The fact that the studies on the travels and universities of Paracelsus had already been published by himself does not seem to disturb de Telepnef. Nevertheless, there must be a limit to the number of times that something can be repeated in print.

J. Strebel discusses the Rosicrucians in relation to Paracelsus; whether the pictures of Paracelsus attributed to Augustin Hirschvogel are actually his work; the views of Plotinus and Paracelsus on horoscopy and fate; and the views of Paracelsus on certain forms of magic.

Other papers in this volume consider Hohenheim's comments on the Hippocratic Aphorisms (L. W. von Brunn), the pathology of Paracelsus (E. Scheidegger), Paracelsus and Schopenhauer (R. Herbertz), and the social and economic views of Paracelsus (K. Bittel). The last of these papers is the most interesting because it deals with an aspect of Paracel-

[14] *Nova Acta Paracelsica. III. Jahrbuch der Schweizerischen Paracelsus Gesellschaft, 1946.* Verlag Birkhäuser, Basel 1946.

sian thought that has been almost completely overlooked, especially by medical historians. The social consciousness and social program of Paracelsus are based on his views of society that derive from the world-picture of the Middle Ages. Hohenheim's program and social ethos are not far from that of a man like Thomas Münzer, and he influenced the German mystic Weigel, especially in his views on the ethos of labor.[15] It is interesting to note that forty-five years ago attention had already been called to socialist elements in the ideas of Paracelsus (Menrad: "Socialistisches in den Schriften des Paracelsus," *Dokumente des Socialismus* ([1902] 224-26), but this hint has never really been followed up. Bittel provides an interesting sketch of certain elements in Hohenheim's social thinking, but a full-dress study of the subject is certainly needed.

With the third volume of the *Nova Acta Paracelsica* this review comes to an end. In general, we may say that of all these publications, only Lindroth's study adds something clearly new to the historical study of Paracelsus and the Paracelsian doctrines. Nevertheless, many of the individual studies mentioned above (e. g. those of Bittel) are stimulating and should be read by all those interested in the life and work of the great Swiss doctor. Finally, we may suggest that English translations, with commentaries, of crucial Paracelsian books are needed for those members of the English-reading public who cannot cope with Paracelsian German. Among these should be at least the *Paragranum,* the *Volumen paramirum,* and *Opus paramirum,* the *Grosse Wundarznei,* the *Philosophia sagax,* the book on tartaric diseases, and one of the syphilis books. Such editions, if properly prepared, would undoubtedly add laurels to Anglo-American scholarship.

[15] Klara Vontobel; "Das Arbeits-ethos des deutschen Protestantismus." *Beiträge zur Soziologie und Sozialphilosophie.* 2 (1946) 62-63.

Medical and Anatomical Terms in the Pentateuch in the Light of Egyptian Medical Papyri

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THE PURPOSE of this article is not to deal with the medical aspect of the subject, but to deal with it mainly from a linguistic viewpoint, and in this way to establish the close connections between Egyptian medicine and the medicine of the Bible, more especially of the Five Books of Moses (the Pentateuch). Throughout my studies of Oriental antiquity, I have realized again and again that archeology alone is not sufficient for the elucidation of the actual intrinsic relations between peoples and civilizations without the help of comparative philology.[1] The study of the linguistic relations, if properly pursued and adequately applied, proves, in most cases, to be even the better and more reliable guide through the obscured ways of early antiquity.[2]

Biblical-Egyptian Relationships

In my study of the medical references and of the names of the diseases in the Five Books of Moses, many of which remained undefined and

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Because of typographical exigencies and a desire to facilitate the reading of the foreign words in this article, a system of transliteration has been adopted which will give a substantially correct pronunciation of the words. A hieroglyphic list of Egyptian words and a list of Hebrew words are given at the end of the article.

[1] A. S. Yahuda, *The Language of the Pentateuch in its Relation to Egyptian* (Oxford 1932); *The Accuracy of the Bible* (London and New York 1934). The following abbreviations are used in this article: Aeg. WB. for *Woerterbuch der aegyptischen Sprache* by A. Erman and H. Grapon (Leipzig 1926-31); AZ for *Zeitschrift fuer aegyptische Sprache und Altertum;* Calice for F. Calice, *Grundlagen der aegyptisch-semitischen Wortvergleichung.*

[2] A few of the Egyptian words treated in this article have been identified with a kin Semitic word by Brugsch in his dictionary and by Ember in AZ vol. 49 and 51 and later collected by F. Behnk in his posthumous work *Egypto-Semitic Studies.* Most of the comparisons were made, however, haphazardly by using elementary Hebrew and Arabic dictionaries without proper knowledge of the languages involved. The most untenable associations were construed by W. F. Albright, and the most reasonable by Ember. From the 948 words registered by Calice, scarcely 50 are tenable, and some of those thought to be Egyptian are Semitic loanwords.

unidentified, I found that the linguistic relationships with Egyptian are so conspicuous in this particular field, that they can only be explained by admitting that the author had the most intimate knowledge of Egyptian medicine in matter and language.

Now, though my purpose is a philological one, I think that my findings are also of interest from a medico-historical point of view. In the first place, they help to determine the stage of medical knowledge and advance in Ancient Hebrew; and secondly, they show the extent of the influence of Egyptian medicine in early times (fifteenth century B. C.) on the Hebrews. Hence the way is opened to identifying the Hebrew names of some diseases, drugs, and remedies, and to determining the origin and meaning of some expressions used in connection with them. But the most important result of my investigations consists in the fact that, whereas the Egyptian medical papyri throw a light on Biblical medicine, it is also possible to find out the true meaning of some Egyptian medical expressions, and to identify some diseases or anatomical parts of the body by establishing the Hebrew equivalent and parallels.

THE BEGINNINGS OF EGYPTIAN MEDICINE

The Egyptian medical papyri have already been quite fully treated[3]; nevertheless, I deem it useful to make a few remarks about our knowledge of Egyptian medicine. Presumably not all those interested in medical history have devoted their attention to "all the ills of Egypt" (Deut. 28, 60).

As in all other parts of the world, the beginning of medicine in Egypt was magical. It was to the magician that one turned for aid in case of illness; and it was from him and his magical spells that one expected salvation. Next to the magician was the priest, the holy man of the temple, who practiced the art of healing, using all kinds of drugs in addition to formulas of incantations and spells. Thus, as is still today the case among primitive tribes and in backward countries, in ancient days, the magician, the priest, and the physician were almost identical.

[3] Cf. *Bull. Hist. Med.* 1 (1933) 237-57: Ranke, *Medicine and Surgery in Ancient Egypt.* Max Meyerhof, "Diseases in Ancient Egypt," in *Ciba Symp.* I, No. 10 (1940) 305. O. Temkin, "Recent Publications on Egyptian and Babylonian Medicine," *Bull. Hist. Med.* 4 (1936) 247-56, 341-47.

Even when the art of healing began to enter the first stages of a scientific discipline, it still remained in the hands of the priests, and the medical schools were housed in the temples near the shrines of protective divinities. And yet there seem to have existed, even in the early days of the Old Kingdom (2900-2500 B.C.), physicians whose treatment of the sick was based on a more or less objective observation of the nature of the illness, and whose therapeutic methods evolved from an accumulation of experience.

One of the first physicians whose name has come down to us is Ny-ankh-Sekhmet. He served as chief physician to King Sahure (2700 B.C.). There was also Khuy, who described himself as "chief of the physicians of Upper and Lower Egypt" and as "court physician to Pharaoh"; he had also the title of being "at the hand of Pharaoh." He lived in the reign of King Teti of the tenth dynasty (2625-2475 B.C.) and was said to be "the superior of the priests of the pyramid of King Teti" and "the interpreter of the secret craft."

Later, Pentu, the physician-in-ordinary to Akhenaton (Amenophis IV. 1375-1350 B.C.) is mentioned as the "first servant of Aton." There are references to other physicians of the Pharaoh, and many an episode is recorded of the part they played in their profession. Upon some stone reliefs are still to be seen certain surgical operations; circumcision or an operation on the fingertips or toes, evidently in cases of swelling under the nails.[4]

Eye diseases were among the most fearful of all the ailments of Egypt, and we find mention of an "eye healer," *Wa'*, who flourished as early as the Old Kingdom (2900-2500 B.C.).[5]

The founder of medical art is said to be Imhotep, court physician of King Zoser of the third dynasty (about 2900 B.C.). He was later deified as "the son of the god Ptah." He was known to the Greeks as Imouthes "the Egyptian Aesculapius," and was called by the Egyptians "the benignant god" (*neter menekh*), and addressed as "the god who bestows life on all men."[5a] Although he was described as "the first discoverer of medicine" (*medicinae primus inventor*), he was by no means the first medical author; in the Ebers Papyrus of the sixteenth century B.C. there

[4] R. Lepsius, *Denkmaeler aus Aegypten und Aethopien* II, 93a and Spiegelburg, AZ 13. III.

[5] *Ibid.* IV, 15d; Sethe, *Imhotep* 20, N.5.

[5a] Cf. Pseudo-Apuleius, *Asclepius*, c.37; Sethe, *Imhotep* 8, N.2.

are vestiges of earlier medical writings, going back to the first dynasty (about 3400-3200 B.C.).

THE MEDICAL PAPYRI

The earliest mention of a medical book in Egyptian records occurs in an episode which tells of Was-Ptah, vizier to King Nefer-ir-ka-Re of the fifth dynasty (about 2700 B.C.). When the vizier suddenly lost consciousness in the presence of the king during the inspection of a new building, incanting priests and physicians were called; they came with "a box (or a leather bag) containing a scroll," a medical scroll containing prescriptions and cures for various ailments.[6]

The largest number of medical texts still extant belong to the New Kingdom (1580-1200 B.C.), though their sources can be traced some 1200 years back to the Old and Middle Kingdoms. By far the most complete and detailed of those which have come down to us are the following: 1. *The medical Papyrus of Berlin,* for the daily use of the practicing physician. 2. The famous *Ebers Papyrus* of the University of Leipzig, undoubtedly the most important; it consists of a sort of textbook of Egyptian medicine and embraces nearly all the branches of medicine, particularly diagnosis and therapy. 3. *The Hearst Papyrus* of the University of California. These three papyri contain prescriptions for all kinds of diseases with no systematic arrangement or inner connection. Only one fairly large section of the Ebers Papyrus, "On Vessels," shows some kind of a systematic treatment of the large blood vessels in connection with the arteries. 4. The *Edwin Smith Papyrus,* containing the largest Egyptian treatise on surgery. This papyrus is of particular interest, because it gives a description of injuries and wounds of some parts of the body, and prescriptions for their treatment. It dates from the times of the Hyksos (eighteenth to seventeenth century B.C.), but some of its linguistic features reveal that parts of it were of an earlier composition in the Middle Kingdom (2200 to 1800 B.C.). And some of its sources go as far back as the Old Kingdom (2900 to 2500 B.C.).

After these preliminary remarks of a general character I come closer

[6] Steindorff, *Urkunden des aegptischen Altertums* I, 42.8; cf. Aeg. WB I, 3.7.8; Yeivin, Kemi 6,64.

to my subject on the relationship between the medical references in the Mosaic books and Egyptian medicine.

The References to Diseases in the Pentateuch

In many passages of the Pentateuch the "Egyptian diseases" are referred to. Ex. 15,26: "I will put none of these diseases upon thee which I have brought upon the Egyptians"; Deut. 7,15: "And the Lord will take away from thee all sickness and will put none of the evil diseases of Egypt, *which thou knowest,* upon thee"; and in chapter 28,60: "Moreover He will bring upon thee all the diseases of Egypt, which *thou wast afraid of,* and they shall cleave unto thee." In the latter passages we notice the emphasis laid on the plagues and diseases of Egypt of which the Hebrews were aware. From this fact we may gather, without doubt, most of the plagues listed in his scourging diatribes, if not all of them, undoubtedly existed in Egypt. Some of the diseases mentioned in Deuteronomy occur also in Lev. 26,16. But there are even more passages in the Pentateuch which are concerned with diseases, disfigurement, and other physical abnormalities: The section dealing with skin diseases, Lev. 13-15; Lev. 21, 16-20 dealing with deformities of the body, which disqualify a priest from participating in the service of the altar; and Lev. 22, 22-25, dealing with blemishes for which an animal is unsuitable for sacrifice. In all these passages we observe a variety of names or terms, the origin and meaning of which are still obscure.

The Medical Knowledge of the Ancient Hebrews

It has been often said that the medical knowledge of the Hebrews in Biblical times was "small in amount and crude in character" (*Dictionary of the Bible, s.v.* Medicine, p.321), whereas Egyptian medicine was in a very advanced stage many centuries before the sojourn of Israel in Egypt, in the middle of the second millenium B.C. But precisely by establishing the close relationships between Hebrew and Egyptian in the field of medical terminology and names of diseases, it is seen that the Hebrews had a wide and perfect knowledge of Egyptian medicine, of its methods and practices.

It will be admitted that there must have been much more medical knowledge behind all these casual references in the Pentateuch, because many of the names of diseases and the few terminological expressions

mentioned in the Pentateuch are coined after Egyptian patterns; they also occur only in the Pentateuch, either in the course of a narrative, or in connection with ordinances and ritual prescriptions, or in diatribes containing threats of divine punishment against the Law. Furthermore, the circumstance that all these references are of such brevity and precision that they have more the character of allusions than of a descriptive nature, makes it clear that they were meant for an audience or readers who had a wider knowledge of the matters alluded to. This fact leads further to the conclusion that the author must have been familiar with Egyptian medicine and medical terminology; and that he drew his knowledge from first hand sources, emanating directly from the Egyptian medical literature.

THE EMBALMING OF JACOB

The first medical reference in the Pentateuch is in Gen. 50, 2 and 4, dealing with the preparation for Jacob's burial. Although the narrator is only concerned with the funeral ceremonies, he mentions some details which are of interest and most illuminating. The statement about the duration of the embalming of the body and the seventy days of mourning is very exact and minutely accords with all we know of Egyptian funeral customs. But there is one expression of particular significance for us, namely, that Joseph gave orders to "the physicians" (*rofe'im*) to embalm the body of his father. This exactly corresponds to the term *sw-nw* for "Physician" in Egyptian, whereas the Hebrew expression *"ḫanot"* for "embalming" corresponds to *"wet"* in Egyptian. The fact that in the case of Jacob the *physicians* and not the ordinary professional embalmers (*wetiyu*) were entrusted with the job, perfectly accords with Egyptian custom, when the deceased belonged to the higher class. Strong evidence is thus provided for the familiarity of the Hebrew writer with Egyptian ritual practices.[7]

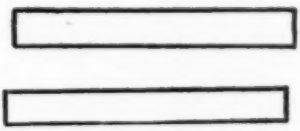
THE STOOL OF THE MIDWIVES

The second reference in the Pentateuch to a close Hebrew-Egyptian relation of a medical nature is in Ex. 1,16. Here the allusion to the seat of confinement is of special importance, because it refers to a typical Egyptian custom in connection with midwifery.[8] The expression is

[7] On my suggestion, Spiegelberg published an article on the subject in *Orientalische Literaturzeitschrift* 26 (1923).

[8] For more details and references to Egyptian sources cf. my *Language of the Pentateuch* 53-56.

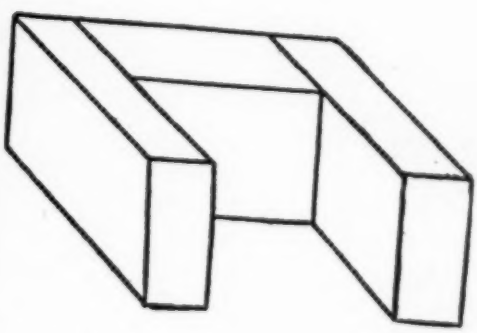
obnayyim a dual form of *oben,* and as it was not intelligible to the Biblical interpreters they translated it "stools."[9] But from Jer. 18,3 it is obvious that the *obnayyim* were of clay, and hence must be translated "two bricks," and from the Egyptian we understand that expression. The seat of confinement was called by the Egyptians *djebeꜣty* "the two bricks." There the woman in labor was seated, and while she was supported by other women holding her arms and sustaining her body, the midwife received the child in her lap from between the two bricks. The passage in question is now clear and reads: 'And the King of Egypt spake to the Hebrew midwives, and said: When you do the office of a midwife to the Hebrew women, then you shall see upon the 'two bricks': If it be a son, you shall kill him; but if a daughter, then she shall live."



a. The two bricks.

b. The improved confinement stool.

c. Confinement stool still in use in Syria and Cyprus.



d. Confinement stool still used by the peasants in Egypt.

Fig. 1. Confinement stools

[9] The dual form *obnay-yim* is from a singular *oben,* for which there is no explanation.

The best illustration of the two-brick stool with the midwives is given in a scene of Queen Ahmes' confinement, preserved in a relief in the temple of Deir-el-Bahri and in the temple at Luxor, showing Queen Mutemwia in the same position.[10]

It is of special significance that the Egyptian expression "the two bricks," in its dual form, occurs in a papyrus of the Hyksos period in the eighteenth century B.C.,[11] that is a few centuries before the Exodus, and that it remained in use until the thirteenth century B.C. Later the dual form was dropped, and the bearing stool was simply called "the brick,"[12] or "the brick of bearing." This is to be explained from the fact that the "bearing stool" was later made more comfortable, consisted of one piece instead of two, and was semicircular in shape, with an opening in front. This is very important, because the dual use of the word in Hebrew can only be explained from the time when the Egyptians themselves used the dual form for the bearing stool. Since it is maintained by Biblical critics and some Egyptologists that the Exodus story could not have been written before the ninth or eighth century B.C., it would be incomprehensible that the Hebrews should have used the dual which was no longer known even among the Egyptians. The grammatical identity in Hebrew and Egyptian thus shows that the Hebrew expression must have been coined at a time when the Hebrews lived together with the Egyptians, before the Exodus in the fifteenth century B.C.

THE FIRST SURGICAL OPERATION

The well known extraction of one of Adam's ribs (Gen. 2, 21) is our first reference to a surgical operation. This episode has a particular significance within the monotheistic frame of the story, as I have shown in *The Language of the Pentateuch* (page 152). But the reference to the operation in the second Book of Moses (Ex. 4,25) is of more interest. In that passage we are told that Zipporah, the wife of Moses, performed

[10] It is characteristic of cultural continuity in Egypt that this confinement stool is still in use among the lower classes in towns and among peasants in the villages (Lane-Poole, *Manners and Customs of the Modern Egyptians*, p. 503; W. S. Blackman, *The Fellahin of Upper Egypt*, p. 63 F.y. 28). In Syria the same shape of confinement stool is used (G. J. Engelman, *Labor among Primitive Peoples*, 3rd ed., fig. 41; Weindler, *Geburtsszenen*, Abb. 7, 13). For Cyprus see Cesnola, *Collection from Cyprus*, Metropolitan Museum of Art, New York, No. 614, fig. from the fifth or fourth century B.C.

[11] *Zauberspruecche fuer Mutter und Kind*, recto 6,6.

[12] Cf. *Language of the Pentateuch*, p. 55, note 2.

the act of circumcision on her child with a "sharp stone" (*Zor*). From one of the reliefs in Saqqarah near Cairo we can see that the same operation was executed with a sharp fire-stone knife. It is this instrument which is meant by the "sharp stone." It is exactly the same one used by Joshua at the performance of the circumcision on all the males after the entry into Canaan: He made for himself "knives of fire-stones" to circumcize the Children of Israel (Jos. 5,3).[13] In the illustration of Saqqarah we have a plastic illustration of the act performed by the wife of Moses and later by Joshua.

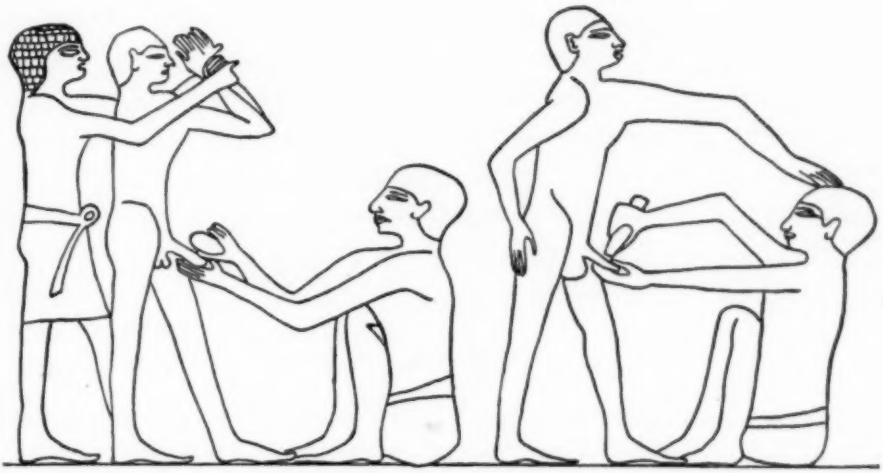


Fig. 2. Circumcision operation with a fire-stone knife.

"Sitting," for Resting of the Patient

In Ex. 21, 18-19, the two *striving together,* it is said that if the wounded man is laid up for some time in bed, the aggressor is bound to pay the expenses for his cure and for "his sitting"; *shebet,* a feminine infinitive from *yashab* "to sit," is used here. Now this expression is literally identical with the Egyptian word *chemeset* (a feminine noun from *chemes* "to sit") employed in medical papyri, on many occasions, for the lying period and perfect rest of the patient during the illness (cf. Aeg. WB III.96ff. sub A III). In the Smith Papyrus c.7. III, 15, p. 183, *chemeset* "sitting" is prescribed for the treatment of a man having a gaping wound

[13] *Zör* (Ex 3,25) is to be rendered "a knife of firestone," as is more explicitly expressed in Jos. 5,3.

in his head and suffering with stiffness in the neck; the patient is to be laid between two bricks to support his skull. The phrase *serukhe-f pw chemese.t* "his treatment is sitting," i.e. perfect rest, occurs several times as the regular expression (cf. *ibid.* c. 45. II,7,p. 149f) on a fracture of the skull. The fact that this word only occurs in Exodus 21,19, and only in connection with an injury; further, that the same word is used in Egyptian in connection with illness caused by injury; and finally, that in Hebrew it has the same grammatical form as in Egyptian—all these circumstances make it clear that the Hebrew word is a terminological expression coined after the Egyptian model and not a mere coincidence.

It is interesting to note that the real meaning of this word was not known in post-Biblical times, and that therefore a discussion arose whether the indemnity was meant to be paid for the loss of time during the period of the illness and convalescence, or whether it was meant for the actual time spent in bed.[14] The latter interpretation is found in the *Jewish Antiquities of Josephus,* 4,8,33., where the assailant has to pay the expense incurred by the injured man for the time of his lying in bed, and for the treatment by the physician.

SKIN DISEASES

Terms and Expressions Coined after Egyptian Equivalents or Loaned from Egyptian. Among the diseases mentioned in Lev. 13-15 and Deut. 28, 21-35, are the names of skin diseases such as boils, tumors, blains, pustules, scabs, warts, burning wounds, and lepra boils. The most characteristic feature is the form *pe'elet* common to them, e.g. *shachefet, dalleqet, zarebet, qaddáchat.* This is precisely the regular form of Egyptian names of diseases, e.g., *shememe.t, serefe.t, webede.t.* An even more prominent feature common to both languages is that these expressions are coined from roots signifying "heat, warmth, burning, inflaming, glowing, consuming," and here the analogy in Hebrew and Egyptian is most apparent: *kaddāchat* from *kadach* "burn," *dalleqet* from *dalaq* "inflame," like *seref.t* from *seref* "be warm," *webede.t* from *webed* "inflame, burn, consume." The Hebrew *zarebet* "burn, boil" is likewise derived from *"zarab"* "burn scorch" (cf. Ez. 21,3). In fact the Hebrew root *zarab* appears in Egyptian *d'ebe.t* for "burning charcoal" (Aeg. WB. V,536), and is perhaps a Semitic

[14] Talmud Babyl., *Baba-Kamma* 84a; *Sanhedrin* 78a; *Kethuboth* 33b.

loan-word.[15] In Hebrew *zarebet* is used for the advanced stage of the healing of a tumor (Lev. 13, 23), or of a boil caused by fire (Lev. 13,28), and most probably designates the resulting scar.

It is possible that either of these parallels may be accidental or explained from the common nature of the disease, and hence derived from a different word of similar meaning. But the cumulative evidence in form and meaning of the many instances quoted is a sufficient indication of a common linguistic basis for their derivation.

"Seeing" for "Examining." For the examination of skin diseases it is said on several occasions that the priest should "look" at the skin or the wound (Lev. 13,3 and 43), or at the scab in the garment (Lev. 13,50 ff.), or at a plague in the house (Lev. 14, 37 ff.). Now from the Egyptian medical papyri we learn that *maa* "looking, seeing," is the technical term for "examining" the illness of the patient (Aeg. WB. II, 8 sub C.IIc). Thus in all medical papyri, the regular phrase addressed to the physician is *maa-chere-k webenw-f* "thou shallt look at his wound" (Smith Pap. C.19, vii, 15, p. 279), or *'ir maa-k s* "when thou lookest at a man ailing in his anus" (*ibid.*, Incant. xxii.11,p.499), even when there is nothing for the physician to "see."[16]

Chabbura "Wound, Injury." Some Hebrew and Egyptian expressions for swelling are formed from roots of analogous basic meaning, e.g. *chabbura* derived from *chabar* "collecting, gathering" is used for "wound, boil," because of blood and matter collecting and gathering there.[17] Similarly in Egyptian, according to Hearst Pap. p. ii, No. 133, Table IX, 11, *khesed* "boil, swelling" comes from *khesed* "gathering" of blood in the swelling (cf. *kesede.t*), a bad, dangerous boil.

Shechin—a Skin Disease. In Ex. 9,9, *shechin* is one of the ten plagues. It clearly means a skin disease of the nature of boils, which, as the text describes them, break forth from the bodies of men and animals with blains. The statement in verse 11, that the magicians could not stand before Moses because of the ailment, suggests that the face and all the limbs were also affected.[18] The same word occurs in Lev. 13,18, for a

[15] A. H. Gardiner, *The Admonitions of an Egyptian Sage.* III, 11 and Brugsch, *Hieroglyph. Demot. Woerterb.* Supplement 1381. In Accadian *Ciribtu* means "consuming fever."

[16] It is very notable that the same expression of "seeing" is also terminological in legal phraseology for "examination."

[17] The translations of *chabbura* in the dictionaries by "blow, strike," and the like, ignore the basic meaning of *chabor* "gathering." It is distinctly differentiated from *makka or nega'* (e.g. Is. 1,6), "blow, strike."

[18] Priests were prohibited from performing their duties when unclean, e.g. with skin disease, vermin, and the like.

kind of boil on the skin, and again in Deut. 28,27, and 35 it is mentioned among other diseases expressly characterized as "Egyptian ills." But it seems that there were two kinds of this skin disease, for in verse 27, it is characterized as *"shechin* of Egypt," whereas in verse 35, it is described as a bad *"shechin* of a sore botch," which especially attacks the knees and the legs and spreads all over the body "from the sole of the foot to the top of the head." It is the same disease with which Job was stricken, (Chapter 2, verse 7): He was smitten with a "bad *shechin* from the sole of his foot unto the crown of his head." From the statement that he took a potsherd to scratch himself with (verse 8), he seems to have had itching boils. As a matter of fact, in the Egyptian medical papyri *sechen* is the name of a disease which the Aeg. WB. IV. 254, describes as a swelling. Only from the Hebrew can the Egyptian name *sechen* be established as referring to a skin disease of itching boils which break forth in blains like blossoms and cover the whole body. From *sechene.t cha'* (Aeg. WB. III.39) "the swelling of the body" we may conclude that it is the same disease as the one described in Hebrew.

Animals were also subject to this ailment. Moreover, the statement that this epidemic was produced by throwing "ashes of the furnace" towards the heaven by Moses (Exodus 9,8), suggests that the disease was caused by dust containing infectious germs, which penetrated the skin and produced boils.

The word *aba'bu'oth* which means "blains like blossoms" is taken from the Egyptian medical literature, and at the same time it explains the Egyptian word. In Papyrus Ebers 104, 2, a skin disease is said to be "something like *pa'pa'y.t."* Aeg. WB. I,504 does not explain it but it is the same word as *ab'bu'oth,* and from it we learn that *pa'pa'y.t* are "blains which break out like blossoms."

Cheres. Another disease mentioned with *shechin* is *cheres* (Deut. 28,24). It is translated by "itch," referring to a kind of eruptive disease like *lichen tropicus.* It cannot be explained from any Semitic word and is very probably the Egyptian word *secher* which occurs in the Smith Papyrus, case 46, as a name of a boil or a tumor (cf. pp. 407 and 413). Aeg. WB. IV, 220 describes it as a kind of wound on the chest. And it well fits into the class of illnesses enumerated in Deuteronomy 28,27.

Techorim. In Deut. 28, 27, the word *'afolim* is replaced by *techorim,* because the former was felt to be obscene, similarly *shagāl,* "to cohabit"

(Deut. 27, 30) is replaced by the less obscene word *shakhab* "to lie."[19] The same substitution for *'afolim* occurs in I. Sam. 5, 6, 9, 12 and 6, 4, 5, 11, 17. Neither the origin nor the exact meaning of both names is known, but they are interpreted by oral tradition as "hemmorrhoids." From Deut. 28, 27 we can tell only that *'afolim* must be a kind of swelling, and since it is mentioned together with *"shechin* of Egypt," a typical Egyptian skin disease, it must also be a common ailment in Egypt. The fact that *'afolim* is replaced by *techorim* indicates that they were tumors in a part of the body which would not be mentioned for reasons of decency. Thus the traditional view that they were piles is quite certain. Now if *techorim* was not felt as obscene, it must be a general inoffensive term, but obviously indicating a substitute for an unspeakable word. This is clearly apparent, for *techorim* is the Egyptian word *dechere.t,* a tumorous itchy disease (Aeg. WB. V, 482 f.; Smith Pap. XVIII, 8, and Breasted's comment on *dechere.t,* p. 475).

Aeg. WB. quotes another kind of skin disease caused by vermin, e.g. lice, *dechere.t sebet* (Pap. Ebers 89, 16, 18; also Aeg. WB. III, 432). This word is also used of "disease" generally.[20] Judging from the passage quoted in I. Sam. 5, 6, 9, 12, it must have been of an epidemic character, spread by mice with terrifying rapidity.

Se'et and Sappāchat, or Mispāchat. The two names of boils, *se'et* and *sappāchat,* occur in Lev. 132; 14,56. All interpreters fail to define the exact meaning of these words or the nature of the disease. Now *se'et* is an infinitive of *nasa* "to raise, to lift high," and means "a raised thing" or "spot," an "elevation." For *sappāchat,* only arbitrary and hazardous etymologies are offered, none of them plausible. In Egyptian we find that *zese.t* is the name of a disease formed from *zesy,* "to raise, to lift up," and a node, like the nob on a branch, and, like node in English, is extended to designate a hard tumor of a rheumatic nature. Whether in Egyptian it also refers to such nodes, I am not in a position to say; but it occurs several times in the Smith Papyrus in connection with stiffness of the neck, and hardening in other parts of the body. Thus it indicates a hard

[19] In II. Kings 18,27 and in Is. 36,12, there is also substitution for obscene vulgar words. In Egyptian, too, there are euphemistic expressions for obscene words, for instance, *pechew.y* (dual) "the two backs" for "buttocks" (Aeg. WB. II, 535); and *pechewy.t* (fem) for "anus" (Aeg. WB. II, 537; Wreszinski, *Med. der alten Aegypter,* II, 223).

[20] Phonetically *d* and *dj* correspond fully to a t in Hebrew. Thus *dechere.t* is rightly reproduced by *Techorim.* In Aram, and Syr. *tehōr* means "strain at stool"; Syr. *techora* is "dysentery" (*zuchar* and *tuchar* in Arabic).

tumor characterized as an elevation, and it is not far from the mark to suggest that the Hebrew expression *se'et* "elevation" is identical with the Egyptian *zese.t*. Such a view becomes even more plausible when we realize that the alternative expression *sappāchat,* which indicates a favorable change and a progress in the process of healing, is identical with the Egyptian *zepeche.t,* the opposite of "elevation." It means "a cavity, a hollow, a cave, a grotto," and it is probable that in the Hebrew *sappāchat* we actually have the opposite of "elevation," indicating the disappearance of the boil in question, which leaves a kind of "hollow" spot instead of the "node." This fact is brought out very clearly in Lev. 13,6 for *sappāchat* is considered clean, and therefore not a disease at all. If it signified a skin disease, it would be difficult to understand why it was not regarded as something infectious and unclean. According to verse 7 it is only deemed unclean and diseased, when the symptom of swelling appears. I think that on the basis of this identification the nature of the boil and its healing process may be established with more or less precision.

Charum. Among the blemishes which make the priest unfit for the altar service is *charum.* The Coptic Bible translates it "crippled nose" in accordance with the traditional rendering. This interpretation seems to be supported by the proper name *charum-af* (Neh. 3,10) which clearly denotes a man with a "crippled nose"; *charum* would thus mean a "cripple."[21]

It is noteworthy that in Egyptian-Arabic *kharam* is used to mean "defect," e.g. *makhrum* "a defective book," with missing pages, or a "fragment." This can well go back to an Egyptian word with a meaning similar to *chaam.*[22]

Shigga'on and Meshugga'. The expressions *shigga'on* and *meshugga'* (Deut. 28,28 and 34) are usually interpreted as "madness, rage." From the Egyptian *sawga* (Aeg. WB. IV, 76) which is of the same root as the Hebrew word, we learn that the meaning is "imbecility, foolishness." It occurs in Wenamun 2,22 where it is said: "What is this foolish (*msh' sawga*) travelling that they caused thee to make" (Gardiner, *Late Egyptian Stories,* p.69). It is probably a Semitic loan word in Egyptian, since it is used by the Prince of Byblos and is written like a foreign word. This is

[21] This name may have its origin in the punishment of cutting noses for theft, Breasted, *Ancient Records* III, 26 note b, and p. 27 note 2. *saaw fenede-f* "for forsaking the good instructions given to them," *ibid.* IV, 219.

[22] The interchange of *aa* in Egyptian and *r* in semitic is quite regular; cf. *cherem,* "net" in Hebrew and *chaam* "fishing with a net" in Egyptian, Aeg. WB. III, 31-32.

the case in our passages, where it is said that they would be smitten by *shigga'on,* that is "imbecility, stupefaction," because of all the terrible or terrifying ordeals they will have to endure.

In I. Sam. 21,14 and 15, where David is said to have feigned being *meshugga',* he did not show madness or rage, he feigned stupidity and imbecility; this is shown perfectly by his behavior as a fool in scribbling on the doors and gates, and by letting his spittle fall down upon his beard. The same applies also to Hosea 9,7, "ridiculing the false prophets," and Jer. 29,26. In the East especially in the interior parts which have not yet been influenced by contact with foreign peoples, it is the imbecile, the fool, or the man who feigns imbecility and behaves accordingly, who is considered by the people to possess an evil spirit. This is exactly the case in the quoted passages from the Bible.

Bohaq. The expression *bohaq* (Lev. 13,39) which occurs in connection with freckles on the skin, is obviously of foreign origin, as it cannot be derived from any Semitic root. As a matter of fact, *baaḳ* in the Smith Papyrus XIV, 1, XV, 21, XVI, 1 designates the color of liquid pus flowing out of a boil or wound.[23] In Egyptian it is a terminological expression and was borrowed by the Hebrews from the medical literature; it means bright white pustules, or a pimple on the skin, and is described in the Pentateuch as harmless, and therefore considered as clean.

Zob and Qa'ah. The word *zob* for "flowing putrid foul stuff" Lev. 15, 1-15 also seems to be akin to Egyptian *saab,* with the same meaning: Pyr. 1257b *chw-sn saab chwaa.t-ḳ ir ta,* "they take care that thy rottenness (decomposed or putrid parts of the body) should not run down to the ground."[24]

Pap. Ebers 86.4 speaks of a discharge *shepe.t n. t met* "which flows from the virile member," obviously a gonorrheal disease. It is the same phrase as in Lev. 15,2 "when any man has a discharge out of his flesh." The English has "a running issue of the flesh," where "flesh" stands for the "virile member."[24a] See below, the section on *basar.*

In Lev. 18,28; 20,22 etc. *qu'ah* "to vomit" is akin to the Egyptian *qaa,* with the same meaning "to spit, to vomit," (Aeg. WB. V. 7).

[23] Breasted conceived "oily" as "quality" not "colour," but there is no substantial difference in the liquid. In the Pentateuch *bōhaḳ* refers to "blains."

[24] Cf. Ember AZ. 51, p. 112 no. 15 and Aeg. WB. III, 420.

[24a] I think that *shpn,* which according to Aeg. WB. I, 443 occurs only once in Egyptian literature, is a mistake for *shp-n* "flowing from," or something similar.

SOME ANATOMICAL TERMS OF EGYPTIAN ORIGIN

Gabbachat "the slope of the head." Anatomical terms of dubious or unknown origin in Hebrew sometimes turn out to be Egyptian loan words and can therefore be identified with certainty. The word *gabbachat* (Lev. 13, 42 and 43) is traditionally interpreted as "baldness of the forehead" and is contrasted to *qarrachat* which is taken to mean "baldness of the back of the head." Now, the interpretation of *gabbachat* is exact, and it is from the Egyptian that we learn that the word *gemeche.t* designates the slope of the head, the temples (Aeg. WB. V, 171).[25] This interpretation is clearly apparent from its use for the seat on which the two frontsides of the royal crown rest and from the pictures of the Egyptian kings wearing the crown, covering the two temples.[26] From this identification of the Hebrew with the Egyptian, we gather that *gabbachat* originally referred to the slope of the head, and then was used for the "baldness of the forehead."[27]

Gid hannashe "the sciatic nerve." Another example is the word *gid hannashe* which occurs only in Gen. 32,33. It is traditionally taken to mean the "vein in the thigh," referring to the sciatic nerve. Now in Egyptian, we find the word *inese.t* which phonetically is the same as the Hebrew word *nashe.* Breasted places it somewhere between the buttock and the belly,[28] but it is the seat of the sciatic nerve.[29] Thus we have here an example of a Hebrew word, *nashe,* which is an Egyptian loan word, and which makes it possible to establish an obscure Egyptian anatomical term.

Qubbā "Breast." The expression *qubbā* (Num. 25,8) is generally interpreted as "belly" or "genitals," a mere assumption without plausible explanation. The man and woman who were stabbed by Pinehas in the

[25] Cf. Erman, *Hymnus an das Diadem,* pp. 46 and 47; *Urk.* IV. p. 200,16; as Erman shows, we must conclude from the Ebers Papyrus 99,7, that *gemehe.t* applies to both sides of the forehead, the "temples."

[26] This may be seen on all the reproductions of the crowns.

[27] Now the meaning of the Hebrew *mi-peat panaw,* Lev. 13,41. "from the side of his face" becomes clear; it refers to the "temples" which are on the "sides of the face." The translation of the Revised Version: "from the part of his head" is a makeshift, for it cannot apply to the "forehead"; the correct translation is "from the side," referring to the "temples."

It should be noted that the end of Lev. 13,55 is a repetition of the end of Lev. 13,42, and that *pechethet* is mistaken for *porachat.* It has no meaning with reference to the "plague of a garment."

[28] For more details see my *Language of the Pentateuch* pp. 245 and 243.

[29] According to Aeg. WB. I. 99, it means "the under part of the leg"; Breasted commenting on case II, 9, p. 153, Smith Papyrus, translates it "legs." H. Holma, *Die Namen der Koerperteile im Assyrisch-Babylonischen,* p. 6.n.3, connects it with the uncertain Accadian word *nusu;* cf. H. A. Tallquist, *Die Assyr. Beschwoerungsserie Maqlu* I-II, p. 126.

qubbā died immediately after the attack. This suggests that the attack was directed against such a part of the body that instantaneous death would result. Now in Egyptian *qaabe.t* designates the breast, or the upper part of it, and this is the part meant by the Hebrew word.[30] Death would follow immediately a stabbing at the breast, but not an attack of the "belly" or "genitals."[31] The verse (Num. 25,8) is to be translated: "And he went after the man of Israel, and thrust both of them—the man of Israel through the breast, and the woman through her breast." The Biblical text is to be corrected accordingly.

Chaze "Breast of an animal." A further reciprocal indentification of an anatomical term in Egyptian and Hebrew is offered by the words *chaaty* and *chaze*. The expression *chaaty* "heart" is very common in Egyptian, but Egyptologists are in doubt about its distinction from the equally common word *'ib* for "heart" According to Aeg. WB. III, 27, *chaat* probably designates "the larger part of that region of the body," in other words it is the anatomical term for the region of the heart. This seems to be confirmed by its use of a slaughtered animal (*ibid.* sub A II), where it is said that *chaaty* should be taken out." Now *chaaty* means literally "the front," and is used also for the "forehead" and adverbially to mean "in front of." Thus *chaaty* formed from *chaat* can only mean that which is "in the front, at the head of," with reference to the region of the heart. This is precisely the case with Hebrew *chaze* which means "in the sight of, the front of" and is extended to "the flesh in the front." It occurs only in the Pentateuch and is used several times exclusively for sacrificial animals (Ex. 29, 26; Lev. 7,30; 8,29; Num. 18,18). Hence we learn from *chaze* that *chaaty* is actually the front part of the body, "the heart," and from *chaaty* we deduct that *chaze* means breast *with the heart* of the sacrificial animal.

The relation between *chaaty* and *'ib* is now clear; originally *chaaty* was used for the part of the heart of animals and also of men as an anatomical term, whereas *'ib* was used spiritually as the seat of life and understanding. Eventually, with regard to men, *chaaty* was also extended to mean "heart," like *'ib,* with all its implications. Nevertheless *chaaty* retained its use as an anatomical term for "heart" in the human body, and

[30] Cf. Smith Papyrus XI, 23, p. 347; from XIII, 17, p. 372 it is clear that *qaabe.t* means the hairy part of the breast. Cf. further the evidence adduced by Breasted for the meaning "breast generally"; see also p. 293.

[31] The vocalization *qubba* in the second instance seems to have preserved the original genuine pronunciation. At any rate it has nothing in common with *qeba* "stomach" in Deut. 18,3.

for the "flesh of the heart" in animals, whereas *'ib* remained exclusively for the heart of men in a spiritual sense.

Mikhnesãyim "Loin Cloth." The Egyptian word *kenes* is an anatomical term and has been a real *crux interpretum*. Aeg. WB. V, 134 defines it as "Koerperteil zwischen After und Genitalien," but according to Dawson it must refer to the "hypogastric region or genitals."[32] Dawson's view is supported by the fact that *kenes* is located between the navel and the phallus. Now it is quite certain that Dawson is right in assuming that *kenes* means the pubic region, but Dawson is not quite sure about the exact part meant. The fact that *kenes* refers to men and women alike, and that it is associated with the phallus (*chnn*), seems to indicate that *kenes* means the pubic bone (*os pubis*) with the hairy part covering it. In accepting this meaning, most of the passages in which *kenes* occurs, become perfectly clear; this meaning is directly supported by a Hebrew word *mikhnesãyim*, a kind of loin cloth prescribed for the priests (Ex. 28,42, etc.). This word occurs only in the Pentateuch and in Ezekiel 44,18 in connection with the garment of the priests. Now while the meaning of the Hebrew word is undoubtedly clear, the origin is very obscure. All efforts to derive it from a Semitic stem fail to give a satisfactory explanation. Considering that the object of the loin cloth was to cover the pubic part of the body, the word *mikhnesãyim* must be a denominative from the Egyptian word *kenes* "the pubic bone." This view is very much supported by the fact that among the Egyptians the loin cloth was tied together between the navel and the phallus just above the pubic bone.[33] We have here again a case where Egyptian and Hebrew mutually elucidate each other, and where a Hebrew word confirms the identity of *kenes* with the pubic bone.

Fig. 3. Loin cloth tied together underneath the navel.

[32] Dawson, AZ 62 (1926) 22.

[33] In Lev. 6,3 it is expressly said that the loin cloth served to cover the *virile* member. Note that *basãr* in this sense occurs only in the Pentateuch and in Ezekiel (16, 26, 23, 20, 44, 7-8) which depends on Leviticus.

Egyptian Terms Explained by Hebrew Usage

Egyptian weded and Hebrew la'ana "gall." There are cases in which a doubtful Egyptian expression may be explained from Hebrew or another Semitic language which has either borrowed it from the Egyptian or translated it and used it exactly as in Egyptian. I will mention here two words held by Egyptologists to be of doubtful meaning. The first is *weded,* which was generally taken to refer to the "brain" and so translated for a long time; but Dawson has shown (*Journal of Egyptian Archaeology* 62, [1926] 21) that it can only mean the "gall."[84] The assumption is based on the Egyptian proverbial phrase, "it is not sweet, it is bitter as *weded,*" which can only refer to the "gall." This interpretation of *weded* is fully corroborated by the Biblical proverbial phrase "as bitter as gall" (Prov. 5, 4), which is literally identical with the Egyptian.

Rir "pus." The second instance is the word *rir* which is connected with *basar* (Lev. 15,3), and describes a veneral disease as "pus" flowing out of the virile member. Now there is a word in Egyptian, a feminine form *ry.t,* with the same meaning, which was read by Egyptologists for many years *ty-t,* because of the similarity of the hieroglyphs *r* and *t.* By means of the Smith Papyrus, Breasted came to the conclusion that the word must be read *ry.t.* Egyptologists may be astonished to find that this word was borrowed by the Hebrews thousands of years ago, and now comes to confirm the correctness of the reading of the Egyptian prototype.

'Atisha. I wish to add another word which may be of Egyptian origin. Job 41, 10, in describing the crocodile, speaks of his sneezing and uses the word *'atisha* from *'atash.* Now the Coptic has preserved the same word *antash* for "sneezing," and it may go back to an Egyptian prototype *'adash.* There is an enigmatic word *'arash* which appears in *Zauberspruche fuer Mutter und Kind,* 3, 9 (cf. Aeg. WB. I. 211) in connection with the nose and must refer to "sneezing." I assume that it is wrongly written for *'adash.* Thus the Hebrew word gives us the clue for it.

Hebrew Terms Identical With Egyptian Equivalents

Basār "flesh" for "genitals." A striking example of the identical use of expression in Hebrew and Egyptian is the euphemistic use of *basār* and *iws* "meat" or "flesh" for male or female genitals. It is so used in Lev. 15, 2

[84] The word *aays* could not be identified as "brain" before the publication of the Smith Papyrus. It was known earlier but wrongly translated.

and 3, for the phallus in gonorrhea, and in verse 19 for the uterus in menstruation (cf. also Gen. 17, 11 ff: Ex. 28,42). In Egyptian too *cha'* "meat, flesh" which in all cases is used like the Hebrew *basār,* is also employed for the phallus in *cha' 'anch*[35] (cf. Aeg. WB. III, 37 sub E).

Chatam. The Egyptian influence is apparent also in the use of *chatam* in the uncommon sense of "closing," like *chetem* in Egyptian. It occurs in this meaning only in Job 9,7 and 24,16, where in both cases *chatam* is construed with *be'ad, sagar be'ad* "to close, shut up," which occurs in many similar passages (e.g. Gen. 7,16; Judges 3,23; I. Sam. 5 and 6).

Expressions connected with illnesses. The regular expression for a wound caused by a blow is *hikka* "to blow, to strike" (Deut. 28,27; 28.35). It corresponds to Egyptian *seqer* "to strike" a wound caused by a blow and is extended generally to wounds.[36]

Another expression in Hebrew which proves to be terminological both in Egyptian and Hebrew is the word *dabaq* literally meaning "to join, to cleave." It is used in Hebrew of an attack by disease (Deut. 28,21 and 50). Parallel with it is the Egyptian *abakh* "to join, to contact" *men abakh m cha'-s* "ailment joined, cleaved to her limbs" (Bentresh Stela, l.q) in the same sense as in Hebrew "befell" (cf. also Aeg. WB. I, 8).

Expressions connected with Birth. The expression in Hebrew *patach rechem* "opening the womb" (Gen. 29,31; 30,22; etc.), for primogeniture, corresponds to the Egyptian *wep khe.t* "to open the womb," with the same meaning (Aeg. WB. I, 300 sub DII and Urk. iv. p. 225).[37]

Another curious expression for "giving birth" is *himlit* (Is. 66,7) meaning "to cause to flee, to escape." This concurs with the Egyptian use of *wa'ar* "to flee," which is also used for "giving birth," and characterizes the quick movement of the child as "fleeing out of the womb." In this connection it may be mentioned that the Hebrew expression *nefel* "the falling one" for "miscarriage" (Job 3,16; Eccl. 6,3) also corresponds to the Egyptian *haay* "to fall down," used for abortion (cf. Smith Pap. p. 285; *haay n iwer* "a miscarriage from a pregnant woman," and Aeg. WB. II, 473 B III).

[35] This stands for *iwf* "flesh" for the sexual organs in Smith Pap, XX, 3, p. 489. Cf. Sethe's commentary on Lesestuecke 57, 17 for the use of *iwf* for the woman's "nakedness, vulva."

[36] The German expression "Wunden schlagen" and "mit Blindheit schlagen" was used first by Luther under Biblical influence.

[37] In Hebrew *pēter rechem* alternates with *patach rechem.* The Accadian *peter ura* "opening of the nakedness" corresponds to *patach 'erwā* in Hebrew, not to *patach rechem* which is identical with Egyptian *wep che.t.*

'Ayin "Eye" for "Color." A very remarkable case is offered in the term "eye" (Lev. 13,55) used to mean "color." Thus it is said of a certain kind of scab, that grows in a garment, that the priest should "look," i.e. "examine," after the garment is washed, to see whether the scab has not changed its "color"; for this term the text has *hafakh 'et 'eino* "changed its eye." Now the use of "eye" for "color" is not only traditionally transmitted from the earliest times, but is obvious from the context itself and is correct beyond doubt. The same term is also used of a plague (Lev. 13,37),[38] but here it occurs in the plural.

In the Smith Papyrus we find the expression *'ir.tyw* "eyes" no less than five times, and in each case it means "appearance, color."[39] In the sentence (XIV, 3, p. 389) *mi ir.t yw temese.t* "like the color of the fruit *temese.t* (or of the *temese.t-seed*)," *ir.tyw* evidently stands simply for "color." Similiarly *ir.tyw* is used quite generally for "color" (VII, 20, p. 281): *desher ir.tyw ire.ty-fy* "red is the color of his eyes," is a play upon words so popular in Egyptian. This passage clearly shows that no definite color is meant by *ir.tym*—neither white (Brugsch) nor red (Gardiner) nor blue (Breasted), but simply "color, appearance."

Exactly as in Hebrew, *ir.tyw* stands for the "color" of the wound (Smith Pap. V, 1, p. 219). In XVI, 14, p. 414, *ir.tyw* refers to the "color" of the boil or the wound, and does not mean "surface" as Breasted translates.[40] This is evident from the fact that *ir.tyw* is put in direct opposition to *temes* "reddish" (XVI, 1, p. 408). Accordingly *nba* does not mean "cold moisture," but a "color," and the explanatory gloss (B.XVI, 15,), "their skin is not hot," does not mean "the moisture is cold," but the "color is faded, or pale"; it is red when the inflammation is more intense. This is made even clearer in XVI, 1 where it is expressly mentioned that there is a reddening." Breasted, in his view that *ir.tyw* can only mean color, admits that he cannot find a satisfactory explanation for such a metaphorical use, and suggests that it may be explained from

[38] The Revised Version has "in his eyes" referring to the priest; but the suffix in *be'ēnaw* obviously goes back to the plagues, not to the priest. Moreover, *'amad* can only mean "if the plague remains in its color."

[39] This is to be emphasized because Aeg. WB. I, 116 dissociates the word *'ir.tiyw "color"* from. *'ir.t* "eye" (I, 106), and considers them to be of different roots. It is furthermore astonishing that Aeg. WB. IV, 412 interprets *ire.tyw saapw*: the "fruit" of this plant, whereas the sense clearly requires "color," as Breasted correctly translated it (Smith Pap. VII, 20, p. 281). The plural in *'ir.tyw* simply denotes "color" as collective, for which several examples can be quoted.

[40] Breasted translates *'irt.yw-sn*, XIII, 22, p. 375, "their surface" because he is not quite sure of its meaning here; but in this case it also means "color, appearance."

the existence of a plant which was called "eye" and used for a certain dye, and then generally employed for "color" and "appearance." The explanation of this metaphor is very simple: Here we have the extension from the act of seeing through the eye to the object seen by the eye, hence "color, appearance." We have an example of a common phenomenon of a transfer from the subject to the object.

Now considering that the medical terminology of the Pentateuch in most cases follows the Egyptian, it is obvious that here too we have a stereotype translation of the Egyptian term. This is strongly supported by the fact that in the Smith Papyrus the same expression appears in the plural and the singular, as in Hebrew and just as in Hebrew, almost literally, in the same phraseology, in connection with the examination of the skin and wounds to see whether they changed or retained their previous color after the first examination. It is precisely this circumstance which offers a conclusive proof for the identity between Hebrew and Egyptian in the use of "eye" for "color." Thus the puzzle of the obscure Egyptian expression is solved by the Hebrew, and this shows again how closely on the one hand the Hebrew follows the Egyptian, and on the other hand how useful the Hebrew is for the understanding of the Egyptian. For here, as in many other cases, the formation of the word and its adaptations to Egyptian must have gone back to the time when Egyptian was in living use among the Hebrews and so common and natural that they simply took over certain expressions and stereotypically translated them into Hebrew.[41]

THE CASE OF A WOMAN ACCUSED OF ADULTERY

The following case shows how current customs in Egypt may frequently be traced back to ancient times. In Num. 5, 11 ff., the account is given of a married woman suspected of adultery.

> It is said that the priest shall take the holy water in an earthen vessel and shall put into it dust of the floor of the Tabernacle . . . and shall charge the woman by an oath. . . . He shall write the prescribed curses in a book, and shall blot them out with the water . . . and he shall give the water to the woman to drink. If she be defiled . . . the water shall enter into her . . . and her belly shall swell and her

[41] In vernacular Egyptian-Arabic *bi'ēnō* "in his eye" is used metaphorically to emphasize the absolute identity, e.g. *el-shay bi'ēnō* "this thing itself." This must have originally meant "its appearance." Hence we have a reminiscent Egyptianism, similar to many other Egyptian-Arabic expressions, as I will show on another occasion.

thigh shall rot. . . . But if she be not defiled . . . then she shall be free and conceive seed.

An incident which occurred in recent years in Khartum, the capital of Sudan, is described by Sir Wallis Budge:

A native who was going to Cairo deposited with a friend a sum of money and asked him to use it for the benefit of his wife if he did not return. The friend accepted the trust and received from his friend a gift of money for his service. The owner of the money went to Cairo, and returned safely to Khartum. When he asked his friend to return him the money which he had deposited with him, the friend denied all knowledge of the matter, and swore by *Allah* that the traveller was mistaking him for someone else. An angry altercation followed, and at length the man who deposited the money went to the Kadi and told him the story. The Kadi summoned the other man, and having heard from him an absolute denial of the charge brought against him by his friend, determined to elicit the truth by means of a bowl of water. He took a sheet of paper and wrote upon it certain verses of the *Koran,* and then having filled a clean earthenware bowl with water, he put the inscribed paper into the water and held it there until all the ink which was still wet on the paper was washed off from it into the water. He then told each man that he was to drink one-half of the water, and solemnly warned them that the judgment of *Allah* would fall upon the liar after he had drunk his half of the water. Each man drank, and the Kadi put the bowl on the ground, and then suddenly the man who had received the money fell backwards and died forthwith.[42]

In ancient times and in backward countries even today, primitive people do not know the pathological causes of such a sudden death, which may occur from heart failure or other troubles due to fear, and attribute it to Divine punishment. Sudden death was made the acid test to prove a person's innocence or guilt. In a naive sense of justice, the guilt and its concomitant remorse inevitably lead to one ultimate end—to Death.

* * * * *

The foregoing considerations have by no means exhausted all the points of contact of Biblical medicine with Egyptian medicine. There are many items in the later Books of the Bible which offer some relationships with, or reactions against, Egyptian. In the field of hygiene, the Pentateuch shows some prescriptions which prove to be a reaction against Egyptian conceptions. In the first place, the emphatic prohibition of consuming blood, because "blood is the soul of the animal" Lev. 17, 11 ff. (also Gen. 9, 4.), seemed to be directed against the Egyptians who used

[42] Sir Wallis Budge, *Amulets and Superstitions* (London 1930) 447.

blood for therapeutic purposes, and recommended even human blood in some magical cases. Another very important restrictive measure is the law of ritual uncleanliness, including all infectious diseases; the strict laws enjoined during menstruation; and especially the isolation of lepers. In Egypt, and probably in other countries of antiquity, such prescriptions were not so strictly observed, or were entirely unknown. To judge from Manetho, the Egyptian pseudo-historian (third century B.C.), the lepers must have moved about freely, and it was only after the epidemic assumed alarming proportions among the Asiatic Hyksos in the sixteenth century B.C., that the Hyksos were driven out of the country.

In conclusion I wish to point out that while Egyptian medical influence is so conspicuous in the Pentateuch, I fail to find any trace of Assyrian or Babylonian influence. Among all the names of diseases and medical terms, there is not one specifically Accadian (a collective name for Assyro-Babylonian languages) loan-word or borrowing through translations into Hebrew, as is the case with Egyptian. Names and expressions which are used in Accadian are common to all Semitic languages and show no distinctive feature of a purely Accadian origin. So for instance, Accad. *garabu,* a kind of scab or boil, is identical with Hebrew *garab* (Lev. 21, 20; Deut. 28, 27) with Arabic *jarab,* and with Aram. and Syr. *garba;* or *ishḳu* "testicles" and Hebrew *eshekh* (Lev. 21,20) is also present in Syriac. Only in later texts when Assyrian domination was spreading in the Land of Israel and Judah, do the penetration of Accadian elements become more tangible and abundant as in the books of Deutero-Isiah, Jeremiah, and more especially in Ezekiel and Daniel which admittedly were composed in Babylonian surroundings.

אֲבָנַיִם	גֶּפֶל
אֲבַעְבֻּעוֹת	נשה
	גִּיד הַנָּשֶׁה
אָשֶׁךְ	סַפַּחַת
בָּשָׂר	עֲטִישָׁה
גַּבַּחַת	עַיִן
גָּרָב	בְּעֵינָיו
דבק	פָּתַח רֶחֶם
דַּלֶּקֶת	צֹר פֶּטֶר רֶחֶם
זוב	חֲרֻבוֹת צֳרִים
זָב מִבְּשָׂרוֹ	צָרֶבֶת
חֲבוּרָה	כַּדַּחַת
חָרוּם	קְבָּה
חֲרוּמַאף	קָרַחַת
חֶרֶס	ראה
חתם	וָרָאָה הַכֹּהֵן
טְחוֹרִים	רוֹפְאִים
עֲפוֹלִים	רִיר
לַעֲנָה	שֶׁבֶת
מלט	שִׁגָּעוֹן
הַמְּלִיטָה	מְשֻׁגָּע
נכה	שַׁחֶפֶת
מַכָּה	שְׁאֵת
יַכֶּךָּ	

Fig. 4. List of Hebrew Words.

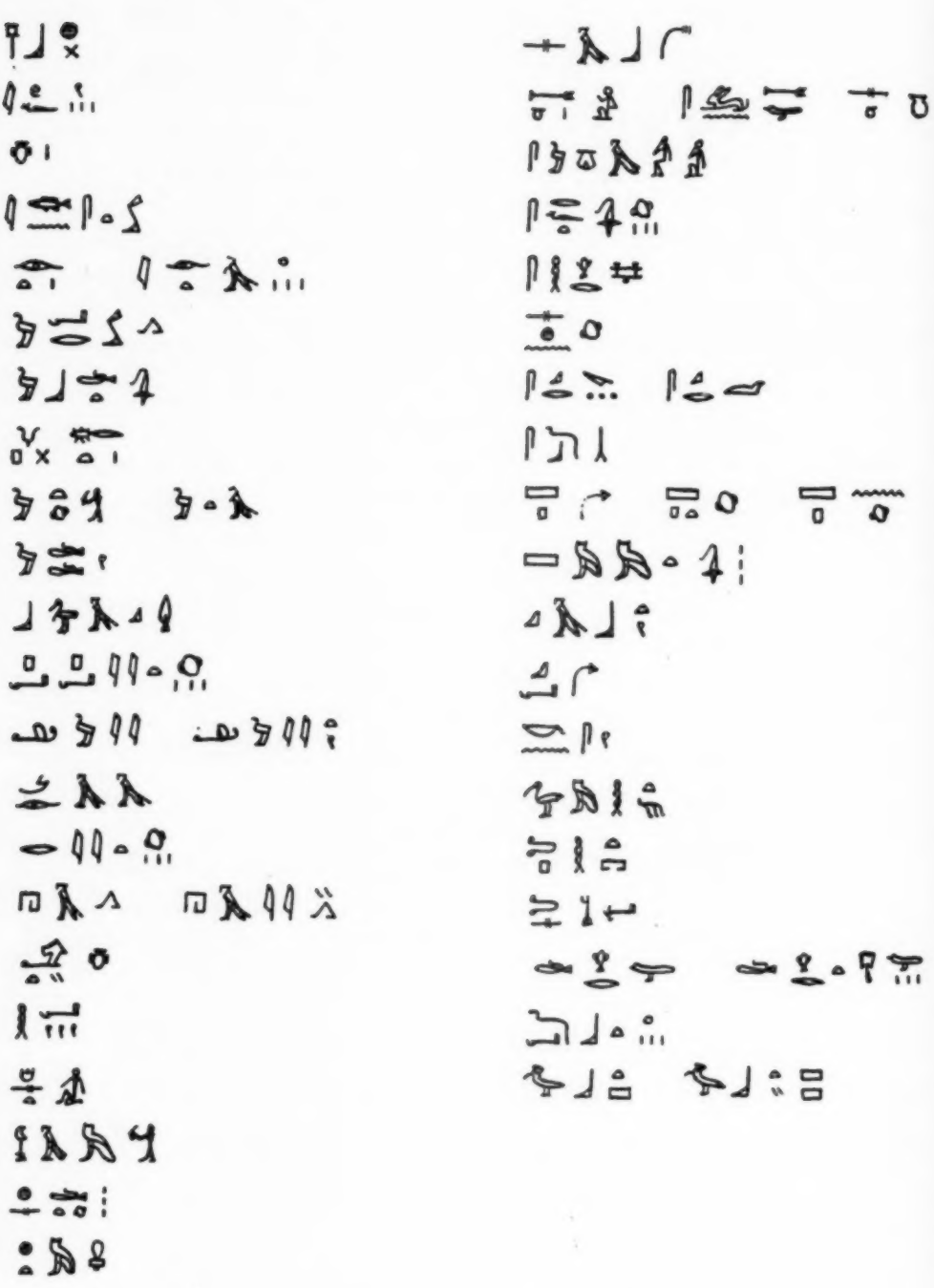


Fig. 5. Hieroglyphic List of Egyptian Words.

Notes and Queries

Edited by RALPH H. MAJOR*

High Fluid Intake in Edema: Eighteenth-Century Experiences

MANY OF OUR present therapeutic measures now based upon good scientific evidence were used empirically in the past. In 1942 Schemm published what appeared to be a revolutionary method of treating cardiac edema.[1] Before the work of Schemm, the management of patients with cardiac edema followed a regime consisting of low salt diet, restriction of water, and the use of ammonia salts and the mercurial diuretics. Schemm showed that fluids up to 4 to 6 liters a day were not only safe but aided in freeing the edema by diluting the sodium ion so that the kidneys could excrete it.[1,2] He also introduced the neutral diet. In his presentation in February 1946, he mentions Sir George Baker's statement in 1772 that a high fluid intake may be beneficial in edema and his advice to physicians to "indulge the patient to the utmost, a limited permission may be pernicious."[3]

In the *Medical Transactions* published by the College of Physicians in London, one finds in Chapter XVII a paper by George Baker, M.D., read at the College in September 1771.[4] Baker points out "that species of dropsy which is called ascites, is too frequently found to be incurable." He also makes the statement that "there is no maxim in physic of greater antiquity or one which has been more universally adopted, than that to abstain, as much as possible, from drinking, is the first step toward the cure of dropsy." He points out the natural appetite is the lead in the curing of some diseases, and in dropsy usually there is a great thirst that should be satisfied.

He then publishes several letters from different surgeons describing patients who had been successfully cured of their ascites and edema by satisfying their thirst.

The patient described by Mr. Rhodes was a farmer near seventy years of age. He had tried various medicines without relief and believing he was going to die, he thought he would end it all quickly; so he drank all he could, three quarts of cold water every twenty-four hours. In addition he had only a little biscuit with butter for about two weeks. At the end of this time he looked

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[1] F. R. Schemm, "A High Fluid Intake in the Management of Edema, Especially Cardiac Edema, I. The Details and Basis of the Regime," *Ann. Int. Med.* 17 (1942) 952-69.

[2] Schemm, "II. Clinical Observations and Data," *Ann. Int. Med.* 21 (1944) 937-76.

[3] Schemm, "A High Fluid Intake in the Management of Edema," 66, 2 (1946) 50-54.

[4] George Baker, "Several Extraordinary Instances of the Cure of the Dropsy," in *Medical Transactions* published by the London College of Physicians, 2 (1786) 235-58.

much worse and immediate death was expected. However, diuresis began and continued for a week, and he made a complete recovery for four years.

The patient of Mr. Samuel Hunn was a young lady, aged twenty, who had generalized anasarca. Purgatives and diuretics gave only temporary relief. She developed marked swelling and was confined to her bed with an insatiable thirst. She finally started drinking large amounts of beer, milk, and water and continued to do so until the swelling entirely disappeared.

Mr. Geach, a surgeon at Plymouth-dock, stated in his communication that for some years he had withheld fluids from dropsical patients. He justified his method of liberal fluids in the treatment of ascites by citing a patient, Mrs. Hawker, aged 39 years, who became dropsical after labor and refused tapping. Her thirst was satisfied with large quantities of well-fermented cider, which was followed by the passage of large amounts of urine and complete recovery from the edema. He also mentioned another dropsical woman of Cornwall who was successfully treated with "the free use of rough cyder."

Another surgeon, a Mr. John Mudge of Plymouth, tells of John Yeab, aged fifty years, who was markedly edematous in the year 1756. He was cured without medicine by drinking new cider, five to six quarts a day, and finally discharged nine to ten quarts of urine in twelve hours.

Thus after nearly two hundred years, with the help of Schemm, we who are treating edema with liberal amounts of fluid are catching up with our forbears.

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A Renaissance Forerunner of Pavlov

A STRIKING PARALLEL to Pavlov's experiment on the conditioned response in dogs is found in John Caius' *De canibus britannicis libellus* (1570), translated by Abraham Fleming as *Of Englishe Dogges* (1576). Pavlov's conclusions (*c.* 1900) had been foreshadowed by Locke (1701), Leibnitz (1704), Robert Whytt (1751), Erasmus Darwin (1794), Victor Cousin (1829), and William James (1890).[5] Caius' remarks, however, are noteworthy for their date and for their similarity to those of the Russian scientist. Describing the shepherd's dog, he says:

> Either at the hearing of his masters voyce, or at the wagging and whisteling in his fist, or at his shrill and horse hissing [he] bringeth the wandring weathers and straying sheepe into the selft same place where his masters' will and wishe is to haue them, wherby the shepherd reapeth this benefite, namely, that with little labour and no toyle or mouing of his feete he may rule and guide his flocke, according to his owne desire. . . . And

[5] Husley Cason, "Conditioned Reflex or Conditioned Response as a Common Activity of Living Organisms," *Psycholog. Bull.* 22 (1925) 446; Franklin Fearing, *Reflex Action: A Study in the History of Physiological Psychology* (Baltimore 1930) 80.

somtimes the straying sheepe, when no dogge runneth before them, nor goeth about & beside them, gather themseues together in a flocke, when they heere the sheepherd whistle in his fist, for feare of the Dogge (as I imagine) remembring this (if vnreasonable creatures may be reported to haue memory) that the Dogge commonly runneth out at his masters warrant which is his whistle. This haue we oftentimes diligently marcked in taking our journey from towne to towne, when wee haue hard a sheepherd whistle we haue rayned in our horse and stoode styll a space, to see the proofe and triall of this matter.[6]

Both Caius and Pavlov, dealing with animals, mention an aural signal in conditioning, the concomitant features being removed. Caius (b. 1510) is well-known as the originator of the first establishment in England for human dissection,[7] as the first to introduce the study of anatomy into England and to teach it publicly,[8] as the author of the first original work on an epidemiological theme by an English physician,[9] and as the editor of Hippocrates and Galen and other medical writers.

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QUERY No. 11. What is the connection of the Countess Anna de Chinchon and cinchona bark?

I recently read in *Antimalarial Drugs,* a report issued by the National Research Council Division of Medical Sciences (Washington, D. C. 1944), page 1, the following: "The name 'cinchona' officially introduced by Linné, is due to a legendary connection of the drug with the Spanish Countess of Cinchon who, however, neither used it herself nor introduced it into Europe." This statement was prompted by Haggis's article, "Fundamental Errors in the Early History of Cinchona," *Bull. Hist. Med.* 10 (1942) 417-59.

It will be seen that from this source also comes the following statement by Lourie, "A Sketch of the History of Chemotherapy," *Brit. Med. Bull.* 4 (1946) 244:

It seems that we must now abandon the oft-told tale of the introduction of cinchona-bark into Europe by the wife of the Viceroy of Peru, the Countess Anna de Chinchon, after she had been cured of malaria in Lima about three hundred years ago. It now appears that this lady died in Spain before her consort went to South America. The Viceroy's second wife, who was with him in Lima, never suffered from malaria; furthermore, she died on her way home, and, therefore, cannot be credited with having brought back the bark herself (Haggis 1941).

Thus we have two scientific monographs following the lead of Haggis.

[6] *Works,* ed. E. S. Roberts (Cambridge University Press 1912) 21-22.

[7] John Aikin, *Biographical Memoirs of Medicine* (London 1780) 103-36.

[8] William Munk, *Roll of the Royal College of Physicians of London.* (London 1878) 1. 45.

[9] *A Boke . . . against the . . . Sweatyng Sicknesses* (1552), ed. A. Malloch (Scholars' Facsimiles and Reprints 1937).

I now quote the actual statement upon which the original and supposedly erroneous belief is based:

> Joseph de Jussieu, who visited Loxa in 1739, relates that the use of the remedy was first made known to a Jesuit Missionary, who being attacked by "intermittent" was cured by the bark administered to him by an Indian cacique at Malacolas, a village near Loxa. The date of this event is not given. The same story is related of the Spanish corregidor of Loxa, Don Juan Lopez Canizares, who is said to have been cured of fever in 1630. Eight years later, the wife of the Viceroy of Peru, Luis Geronimo Fernandez de Cabrera y Bobadilla, fourth Count of Chinchon, having been attacked with fever, the same corregidor of Loxa sent a packet of powdered bark to her physician Juan de Vega, assuring him of its efficacy in the treatment of "tertiana." The drug fully bore out its reputation, and the Countess Ana was cured. Upon her recovery, she caused to be collected large quantities of the bark, which she used to give away to those sick of fever, so that the medicine came to be called "Polvo de la Condesa," i.e., "The Countess's Powder." It was certainly known in Spain the following year (1639), when it was first tried at Alcala de Henares near Madrid. (Flueckiger and Hanbury, *Pharmacographia,* 1879, 342.)

Now this does not say that the first wife of the Count of Chinchon went to South America, was treated there, much less died there. The powder was sent, evidently to Spain, where this lady resided; she used the powder, treated the poor with it, and evidently died in Spain. Her husband married again and lived in Peru with his second wife who died on her homeward voyage. It is possible that Haggis is quoting authorities unknown to me; otherwise certainly in the story given there is nothing to contradict the long cherished and widely accepted association of the name cinchona with that of the Viceroy's first wife.

THOMAS DRAPER

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Final Edition of "Courage and Devotion Beyond the Call of Duty"

The final edition of the book, "Courage and Devotion Beyond the Call of Duty," which is composed of official awards and citations received by U. S. medical officers during World War II, is now being prepared by Mead Johnson and Company, Evansville, Indiana.

Any physician, who has not already done so, should write to Mead Johnson and Co. advising them of the awards he has received and also send a typewritten or photostatic copy of his citations. The following additional information would be of assistance in compiling the material for this book: Present rank or rank at time of discharge. Branch of service. From what university and in what year M. D. degree was received. Date of entry into the service.

The Johns Hopkins University's First Doctorate in Medical History

The first Ph.D. degree in the history of medicine ever awarded by the Johns Hopkins University was conferred in June on Mrs. Ilza Vieth. Dr.

Vieth wrote a translation and commentary on an early Chinese work, *The Yellow Emperor's Classic of Internal Medicine;* she held a Fellowship from the Rockefeller Foundation while working on the dissertation. She has a broad linguistic and historical training, and studied medicine at the Universities of Geneva and Vienna. She will work this fall for the Library of the Johns Hopkins University, as a specialist in Chinese and Russian.

Chymia: Studies in the History of Chemistry

The University of Pennsylvania Press will initiate shortly an annual in the history of chemistry, entitled *Chymia,* the first publication of its kind in the United States. Each volume will consist of about a dozen articles in the field, approximately 4000 to 5000 words each, contributed by outstanding scholars from all over the world. Papers on a great variety of subjects will be included and publication may be in French, German, Spanish, Portuguese, and Italian, as well as English. *Chymia* is sponsored by the Edgar F. Smith Memorial Collection at the University of Pennsylvania, which is renowned as a depository of rare books, manuscripts, prints, and other items of interest in the development of chemistry. Publication will be under the direction of a board of American editors, with a large group of consulting editors of international prominence. Requests for further information on *Chymia* should be addressed to Eva V. Armstrong, Secretary of the Board of Editors, Edgar F. Smith Collection, Harrison Laboratory, University of Pennsylvania, Philadelphia 4.

Book Reviews

Religion of a Scientist. Selections from Gustav Th. Fechner, edited and translated by WALTER LOWRIE. New York: Pantheon Books, 1946. 281 pp.

THIS book, as Mr. Lowrie himself says, is a literary innovation, "a jumble of biography, exposition and quotation" (p. 11). Eighty-one of its two hundred and seventy-seven pages contain an introduction to Fechner and his philosophy; the passages translated are interspersed with notes. The quotations themselves are condensations, a "Reader's Digest" of Fechner (p. 79). I do not hesitate to say that the experiment fits the case and that it has been carried out successfully. Fechner's complicated writings need explanation; even their author seems to have recognized the need for a compendious treatment of his themes (p. 80). And with me, at any rate, Mr. Lowrie has achieved his aim, that the reader of his selection turn to Fechner's own works (p. 11). Having read the book under review I could not help studying Fechner.

As for the texts chosen, the first chapter (pp. 85-126) is taken mainly from *The Three Motives and Grounds of Faith, Concerning Souls,* and *The Daylight View* (p. 80). The second chapter (pp. 129-158) is made up of excerpts from *Concerning Souls* (p. 129, n. 1); the third (pp. 161-212) from *Nanna* (p. 161, Fechner's illustrations drawn from contemporary botanists have been replaced by material from Francé's *The Soul of Plants*); the fourth (pp. 215-277) from *Zend-Avesta* or the *Living Word* and *The Daylight View*.[1] It is gratifying that Mr. Lowrie has not duplicated earlier translations although, as a consequence, the *Little Book of Life after Death* and significant passages of the *Zend-Avesta* had to be omitted (pp. 215, 250, n. 2).[2] The renderings of the latter, and those of Mr. Lowrie are thus complementary, and with the exception of Fechner's writings on merely scientific questions, on aesthetics, and on psychic research (pp. 29, 48, 73), sections of all of his main works are now available in English. Mr. Lowrie's contribution to this welcome achievement merits high praise. The translations are done with that skill and felicity of expression which one has come to take for granted on the part of the translator of Kierkegaard. In spite of abbreviation and condensation, the peculiar tenor of Fechner's philosophizing has been retained, as any test made at random will reveal. Mr. Lowrie's modest awareness of imperfection (p. 81)

[1] Only the last chapter gives in each case the exact reference for the passages borrowed. For convenience's sake this should perhaps have been done also in the preceding chapters.

[2] G. T. Fechner, *Life After Death* (Introduction by W. James; Prefatory Note by John Erskine, New York, Pantheon Books 1943).

I can counter only with the statement that it is hard to imagine how anyone could have performed better the task he has set for himself.

The spirited introduction gives a clear picture of Fechner's life and of his position among his contemporaries. His most important philosophical concepts are analyzed, notably that of analogy (pp. 74 ff.), and his physical and psychological studies are also considered (pp. 29, 45 ff.). His psycho-physics is defended against those who ignore the psyche and yet impertinently call themselves psychologists (p. 48). Especially interesting is a short history of Darwinism (pp. 56-70) which evaluates Fechner's acceptance of, and objection to, Darwin's doctrine within the context of the development of biological theories from Darwin to the present time. This digression, together with the remarks on "Scientists in General" (pp. 55 f.) will, I think, stimulate reflection in everybody concerned with the influence of science on thought in general.[3]

But Mr. Lowrie's book is written not only, nor even mainly, as a representation and historical appreciation of Fechner's views; it is intended to give much more. To Mr. Lowrie, who is a theologian, Fechner is the mouthpiece through which he wishes to deliver a message to the scientists; through Fechner he says what he himself could not express in scientific categories which one must use if one is to reach the scientists (pp. 11 f.). *Religion of a Scientist* really is *Religion for the Scientist*. Mr. Lowrie wishes to show that religion and science, and more specifically that Christianity and science, are reconcilable rather than antagonistic. Fechner is the example of the scientist who qua scientist felt compelled to embrace religion, an example to be followed by others.

To be sure, Mr. Lowrie does not commend Fechner unreservedly. Although as a critic of modern Protestantism, which in his opinion is too sober, he is not offended by Fechner's exuberance, nor, it seems, by his hedonism or paneudaimonia and his pantheism (pp. 71-73), he admits that Fechner's interpretation of the dogma is "thin" (p. 54), that he furnishes one with no creed (p. 35). Mr. Lowrie also objects to Fechner's theosophy. In short, in some respects Fechner believes less, in others he believes more than Mr. Lowrie would expect from a Christian (pp. 10 f.). Yet the index lists Fechner without qualification as a Christian, with reference to the quotation given on page 265, and the first page of the introduction states that the concern of the book is with Fechner's "religious thoughts" which are presented "as the most eminent modern instance of 'natural religion,' religion arising from below up, which is within reach of the scientist who will stretch upward with the hope of apprehending the revelation which is from above down." And the words

[3] For a second edition of the book which I hope will soon appear, I note two slips: Feuchtersleben's book *On the Dietetic of the Soul* cannot have engaged the attention of Kant (p. 25); the proponent of Pragmatism in England was Schiller, not Schilling (p. 116).

religion, religious speculation, religious works, devoutly religious appear again and again in connection with the analysis of Fechner's thought (e.g. 43, 46, 53-55).

Much as I agree with Mr. Lowrie's thesis that Fechner tried to reconcile science with religion, and with Christianity in particular, I am not certain that his interpretation has done justice to the peculiar brand of religion and of Christianity to which Fechner subscribed. To take first the question of Fechner's adherence to the Christian faith, it is true that in his own opinion he remained a Christian (p. 285), that Christ to him was a great teacher (pp. 265-70). But he himself also stressed the great difference between his Christian belief and that of established Christianity (pp. 125 f.), and he did so with good reason. For The Daylight View does not only imply a "thin" interpretation of the dogma, a somewhat heretical interpretation of the Bible (pp. 270 ff.), it also necessitates the abolition of "many oppressive dogmas" (p. 242), among them such basic tenets as that of original sin, of Christ's death for the salvation of mankind (*Tagesansicht,* p. 6).[4] Disregarding those rather heterodox views which Mr. Lowrie finds reconcilable with Christianity, it will seem to most people that merely on account of the dogmas repudiated Fechner's Christian faith is Christian in name alone, a faith which is deprived of all and every characteristically Christian trait. Even Fechner himself made no claim other than that none of his teachings were opposed to the teaching of Christ (p. 265).[5]

As for Fechner's religion, he asserts indeed that his philosophy goes beyond philosophy, and in its ultimate principles is religion. But it is significant that he adds: "Even where The Daylight View is more than philosophy, it still is not opposed to philosophy," while orthodox religion is anti-philosophical (*Tagesansicht,* p. 54). And his concept of God is religious only in the sense of the last definition of the word religion as given in Webster's dictionary: "awareness or conviction of the existence of a supreme being, arousing reverence." As he once says, we must in some way conceive of the spirit as a whole, and for this spirit "we will have to seek a name . . . and will find no

[4] *Die Tagesansicht gegenüber der Nachtansicht* (2nd ed. 1904). If I am not mistaken, the passage referred to is not to be found in Mr. Lowrie's book, nor are the following references which I quote from Fechner's works directly.

[5] Cf. the interesting article of W. Bölsche, *Fechner* in *Hinter der Weltstadt* (1901) 259-347 (an apparently almost forgotten appreciation of Fechner), esp. pp. 273, 339. Bölsche perhaps goes too far in assuming that for Fechner there remains only a kind of moral Christianity which every educated person would and could accept (yet cf. *Tagesansicht,* p. 59, n. 1, where Christ's doctrine is essentially defined as the teaching of human brotherhood). At any rate, in spite of Fechner's agreement with certain interpretations of Christianity current in his own time (p. 270, n. 1), one can ask whether the name Christianity is really meaningful with regard to his attitude. For the opposition of orthodox Christians to Fechner, cf. Kuntze's biography (Bölsche, p. 274).

other than God" (*Tagesansicht,* p. 83).[6] Certainly, Fechner maintains that the worst religion is better than no religion. His own religion, however, is of the vaguest, even though it includes prayers (pp. 241 ff.); it is at best a philosophical religion. Also that religious belief which in Fechner's opinion has been agreed upon in the course of history does not cover more than the bare outlines of a religion (p. 101).

Fechner's position is perhaps brought out most clearly in his theory of faith. His philosophy becomes religion, as he says, because it does not dispense with faith (*Tagesansicht,* p. 54), and he distinguishes three motives of faith: the historical, the practical, and the theoretical (pp. 85 ff.). Whatever the value of the historical and practical motives which require faith (p. 124), the theoretical principle "can serve to support and complete the others without needing their service" (p. 117). Briefly stated, it teaches that "one believes what experience and reason impel one to accept as true" (*ibid.*). This theoretical faith is a philosophical argument which goes beyond the present-day standpoint of Christendom (p. 126); although not of predominant importance (*ibid.*), it justifies faith (p. 91) and gives in the genuine sense the grounds for believing (p. 95). Mr. Lowrie thinks that Fechner's theory of faith is only apt to make plausible the faith which he actually had, "to support that ardent faith" (p. 78). It seems to me that Fechner could and would have rejected faith had it been contrary to philosophy. His reason did not make faith plausible, it made faith acceptable to him.[7]

To speak more generally, Fechner was a scientist who turned philosopher. As a philosopher, intent on arguing scientifically or at least on remaining within the limits of strictly scientific presuppositions, he believed it possible to prove the existence of God. As a philosopher, he elaborated a system which in certain respects agreed with religion, even with Christianity. As a philosopher, he re-introduced the principle of faith. For Fechner held that man has immediate knowledge only of himself and that this immediate knowledge is the starting point of all mediated knowledge and of the faith that is based on it (*Tagesansicht,* p. 19). In the last analysis, he was, according to his own admission, an adherent of objective idealism (*ibid.* 240).[8] Properly speaking,

[6] This quotation is taken from a chapter called "Sprachliches" (pp. 82-85). Here Fechner says: "The Daylight View must divorce itself from many old concepts, consequently also from many word usages" (p. 83). He was apparently aware of the confusion of terms to which his philosophy might give rise.

[7] On the basis of this supposition one can also solve the biographical problem posed by Mr. Lowrie, namely that Fechner's belief started in a period in which he was an atheist and contemptuously discarded the traditional motives of faith. It is quite unnecessary to seek for "some other motive to set faith going again—a motive which is not expressly included among 'the three motives and grounds of faith' " (p. 78), that is, faith itself. Cf. also *Zend-Avesta* 3. 353. Mr. Lowrie's formulation at the beginning of p. 54 states the relationship between knowledge and faith more correctly.

[8] Cf. again Bölsche, *op. cit.* 342-44. Mr. Lowrie's reasoning on this point (pp. 45 f., 157) seems to me faulty.

his doctrine is metaphysics. F. Paulsen quite rightly says of Fechner that he opposed to a merely physical picture of the world a metaphysical one.[9]

Instead of emphasizing the terms religion and Christianity and faith, then, the interpreter, I feel, should emphasize their specific, philosophical modifications as made by Fechner. This seems the more important since the relationship between philosophy and religion, between reason and faith, today is the subject of an ardent debate in which historical parallels are used abundantly. Fechner's teaching will often be referred to, now that attention has again been drawn to it in so masterly and vivid a fashion. Caution therefore is advisable in order to prevent misunderstandings. Mr. Lowrie himself has clearly set forth the difference between Fechner's theory of faith and that of James.[10] It is with the same wish for historical accuracy that I have ventured to raise some objections to his interpretation of Fechner. I do not have any intention of belittling the importance of the argument as such, and I fully agree that Fechner makes timely reading.

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University of Washington

CHARLES BROOK. *Battling Surgeon.* Glasgow: The Strickland Press, 1945. 176 pp., 1 illustration. 2/6 d.

J. M. D. OLMSTED. *Charles-Edouard Brown-Séquard. A Nineteenth Century Neurologist and Endocrinologist.* Baltimore: The Johns Hopkins Press, 1946. 253 pp., 2 illustrations. $3.00.

GERALD B. WEBB AND DESMOND POWELL. *Henry Sewall. Physiologist and Physician.* Baltimore: The Johns Hopkins Press, 1946. ix+191 pp., 15 illustrations. $2.75.

IN point of time, the lives of the three physicians with whose biographies we are concerned here span a period from 1795 (when Thomas Wakley was born) to 1936 (when Henry Sewall died). Having lived through one of the most eventful periods in medical history, during which medicine as we know it finally came into being, the biographies of these doctors reflect characteristically

[9] Cf. Paulsen's introduction to G. T. Fechner, *Uber die Seelenfrage* (2nd ed. by E. Spranger, 1907) p. IX.

[10] Pp. 116 f., 107, n. 1. The divergence of Fechner from Kierkegaard and modern existentialism is implied by Mr. Lowrie's statements, pp. 12 f.

various fields of medical activity and the important changes that occurred in them.

The name of Thomas Wakley does not adorn the peaks of medical fame, yet in the long view his activities and achievements loom greater than those of many of his contemporaries whose names have come down to us attached to some clinical description. His life story, a dramatic one, was told once before, in 1897, by S. Squire Sprigge, but apart from the fact that he was the founder of the *Lancet,* his name means little to the present generation. For this reason, Dr. Brook's graphic picture of the life of this medical reformer of a century ago is all the more welcome.

Thomas Wakley, the son of a prosperous farmer, was born on July 11, 1795. At the age of fifteen, he decided to become a member of the medical profession, and was apprenticed to an apothecary. At the beginning of the nineteenth century the social structure of the English medical profession was hierarchical, with the physicians at the top, and below them, in descending order of prestige, the three inferior groups of surgeons, apothecaries, and druggists. The apothecary may be described as the general practitioner of the period, for as Adam Smith had already said in 1776, he was "the physician of the poor in all cases, and of the rich when the distress or danger is not very great." Young Wakley was ambitious, however, and wishing to become a surgeon studied for varying periods with two surgeons. In 1815, just after the battle of Waterloo, he arrived in London to complete his studies and to obtain his surgeon's diploma. Two years after his arrival, in October 1817, he passed his examination for membership in the Royal College of Surgeons.

Wakley's experience as a student had irritated and angered him, and there would seem to be small doubt that his later militancy against the "upper crust" of the profession derived from this period. Nepotism and favoritism in the hospitals were rampant, but what rankled most was the bitter realization that because he had been unable to pay exorbitant fees to be apprenticed to a leading surgeon, he was condemned to spend the rest of his life among the inferior rank and file of the profession. It was toward the elimination of this sharp class cleavage in the medical profession, and to the elevation of the status of the medical rank and file that Wakely directed a considerable measure of his effort.

On October 5, 1823, there appeared the first number of the *Lancet* with Wakley as editor. In this venture, he was assisted by William Cobbett, probably the most powerful political pamphleteer in English history, and Cobbett's influence is evident in Wakley's early writings. The *Lancet* was soon recognized as a dangerous enemy by the inhabitants of the English medical Olympus. Because he published reports of the lectures of some hospital surgeons and physicians which these gentlemen, with an eye to their class fees, were anxious

to preserve for the exclusive benefit of their students, Wakley was denounced as a literary pirate. During the first ten years of the existence of the *Lancet,* no fewer than ten law suits, six of them for libel, were brought against the editor. Wakley was also outspoken in his denunciation of the abuses of the College of Surgeons, and it is to his strenuous efforts that the reforms later carried out were due.

Equally significant was his political career. In 1835 he entered Parliament as an Independent Radical. Wakley enjoyed the friendship and support, and shared the ideas of such men as Francis Place and Joseph Hume. When he entered the House of Commons, Wakley concentrated his attention on the effects of the recently passed Poor Law Amendment Act, and whenever he found an opportunity he attacked it. He was close to the labor movement of his day, and although Wakley had no official association with the Chartist movement, when the great petition was presented he spoke eloquently in its favor. Also in a speech moving an amendment to the first speech of Queen Victoria from the Throne, he described himself as a representative of labor.

Probably his chief parliamentary work was the introduction, in 1846, of a Bill for the Registration of Qualified Medical Practitioners, and for amending the law relating to the practice of medicine in Great Britain and Ireland. This bill did not become law, but it was the embryo of the Medical Act of 1858.

From the earliest days of the *Lancet,* Wakley had insisted that coroners should be medical men. His agitation was successful, and in 1839 he became Coroner of West Middlesex. At once he set about reforming the whole procedure of the coroner's inquest. Wakley insisted that those who served as jurors should be sober, responsible citizens, that the proceedings at inquests be conducted in an orderly manner, that a post-mortem examination be made whenever necessary. As coroner he was also active in exposing the rigors of the administration of the Poor Law. In 1839, he forced the exhumation of Thomas Austin, a pauper, who having fallen into a boiler in the workhouse laundry had been scalded to death and quietly buried. The jury censured the workhouse authorities; but the workhouse master sneeringly replied: "The jury have found a verdict, but they have not identified the body." Wakley had the last word. "If this is not," he blandly inquired, "the body of the man who was killed in your vat, pray, sir, how many paupers have you boiled?"

Both in and out of Parliament Wakley was always active in the promotion of public health legislation. Through the medium of the *Lancet,* he carried on an active campaign against food adulteration. This war on food adulteration was the last campaign that Wakley waged, for in 1857 deteriorating health led to cessation of his activities. He died in 1862, of pulmonary tuberculosis.

Endocrinology is today one of the active and expanding fields of medicine. Before 1890, however, there were comparatively few publications on the ductless

glands. The man who in large measure put the endocrines on the medical map was Charles-Édouard Brown-Séquard (1817-1894). Dr. Olmsted, who has previously given us excellent biographical studies of François Magendie and Claude Bernard, now gives us an interesting account of the last member of the trio who formed the great tradition of experimental physiology at the Collège de France. Magendie, Bernard, and Brown-Séquard spanned the nineteenth century, and from them came the concepts whose development now occupies much contemporary investigation.

Brown-Séquard, roving physiologist and neurologist, was born at Mauritius, the son of an American father and a French mother. He divided his life between France, the United States, and England, and perhaps for this reason is not as well known in each of these countries as he deserves to be in every one of them.

Brown-Séquard's first major interest was the physiology of the spinal cord, a subject with which his name will always be associated (hemi-section, 1849; hemiplegia with crossed anesthesia, 1850) Later, he became interested in neurology, and from 1858 to 1864 practiced in London as a neurologist. In 1856, Brown-Séquard had been able to reproduce Addison's disease in dogs by the extirpation of the adrenals. Not until 1889, however, when he returned to the endocrine problem, and reported the wonderful effects of testicular extract on his aged body, did he present a general theory of internal secretion, which in the main still holds good.

Olmsted gives a clear and explicit factual account of the professional career of this scientific Ulysses. Here and there, however, are brief references to aspects of Brown-Séquard's career upon which somewhat more light might have been shed. For instance, we are told that Brown-Séquard was "an ardent republican" and that "arms in hand" he opposed the coup d'état of Louis Napoleon (p. 32). It would be of considerable interest to know in somewhat more detail what were the views of Brown-Séquard on political and social questions, whether he actually fought on the barricades, and with whom he associated in political matters. Similarly, we are told that in Virginia he frankly expressed his "great repulsion" to the idea of slavery (p. 82). Here too, more detail would certainly have been in order. Finally, one should like to know what was Brown-Séquard's philosophy of science, if he had one, but again Olmsted leaves us unsatisfied. Aside from these omissions, the biography of Brown-Séquard is a worthy addition to Dr. Olmsted's gallery of French physiologists.

Henry Sewall, physiologist and physician, does not have the claim to fame of a medical reformer like Thomas Wakley, nor did he ever rise as a scientist to the heights of Brown-Séquard, yet for the student of American medical history his life story has an attraction quite its own. Sewall belonged to that generation of American students who in the latter half of the nineteenth

century flocked to Germany to learn pathology from Virchow, physiology from Ludwig, and bacteriology from Koch. These men applied the spirit and the methods that they acquired to their work in America. In so doing, they gradually effected a change in the teaching and advancement of medicine in this country which can only be called revolutionary. It is of the greatest importance to emphasize this fact for in recent years there has been an increasing tendency to regard American medicine as having sprung full-blown from the brow of Jove, rather than as the product of the free interaction of ideas and men.

Sewall was born in 1855, the son of a minister, at Winchester, Virginia, but he grew up in Baltimore. (Among his ancestors was Judge Samuel Sewall (1652-1730) who was involved in the Salem witch trials of the 1690's, and whose *Diary* gives an inimitable picture of colonial life.) Educated in Baltimore and Brooklyn where his father held pastorates, Sewall in 1871 was admitted to Wesleyan. An excellent picture of college life in the 'seventies of the last century is presented in the chapter entitled "Wesleyan Days." Following graduation, Sewall studied with Newell Martin at the newly opened Johns Hopkins University, receiving his Ph.D. in 1879. That same year he set out for Europe where he stayed a while with Sir Michael Foster in England; but soon moved on to Leipzig where he entered the laboratory of Carl Ludwig. From Leipzig, Sewall went to Heidelberg to work in the laboratory of Willy Kühne, who was professor of physiology. The work done under Kühne's direction led to a joint paper published in 1881, and entitled "Zur Physiologie des Sehepithels, insbesondere der Fische."

Upon his return, Sewall was at Johns Hopkins for a while, but in 1881 he was offered the chair of physiology at Michigan. He accepted, and in the spring of 1882 began his work at Ann Arbor. It was during his years at Michigan that Sewall performed his pioneer researches with snake venom—proving the efficacy of immunization to snake venom, and putting it on a sound quantitative basis. This work was published in 1887 in the British *Journal of Physiology*. As Webb and Powell, the authors of Sewall's biography, point out, this work may very well have influenced the thinking of various European workers in the field of immunity.

In 1889, Sewall resigned his professorship because of tuberculosis, and in 1891 settled in Denver, Colorado to practice medicine. Here, he became particularly interested in tuberculosis, and in 1904 was present at the birth of the National Tuberculosis Association.

Henry Sewall lived a long and useful life, dying in 1936 at the age of eighty-one. Gerald Webb and Desmond Powell have presented a vivid account of the life, personality, and achievement of this American physiologist and physician.

GEORGE ROSEN

JAMES ECKMAN. *Jerome Cardan.* Supplements to the Bulletin of the History of Medicine No. 7. xiv + 120 pp. Baltimore: The Johns Hopkins Press, 1946. $2.00

In four compact Latin-titled chapters Dr. James Eckman of the Mayo Clinic presents a modern study of the Milanese physician, Jerome Cardan, whose sixteenth-century fame time has slowly tarnished or dissipated in spite of all that has been written by him or about him.

A review (Dr. Eckman says it is "by no means complete") of the voluminous collection of writings about Cardan's life and activities, from estimates of his contemporaries to an evaluation as late as 1941, is part of the author's aim in the first chapter, *Rei expositio.* He discusses translations of Cardan's autobiography, *De vita propria liber.* He discusses biographies, works of Cardan's detractors and of his far less numerous vindicators, and articles and studies in Latin, German, French, Italian, and English which evaluate Cardan as mathematician, astrologer, physician, magician, atheist, and scientist—in all forty-eight writings. Particularly gratifying is the way Dr. Eckman has run down the facts of the manuscript life of Cardan by James Crossley. There is both pleasure and satisfaction in settling once and for all a matter of literary and historical significance that has been in question for many years.

Cardan's own books, which issued from printing presses over Europe from Rome to London, many of them while the author still lived, and which brought him merited recognition and fame, add to the mass of literary output which causes Dr. Eckman to question the justification of further study of the man. The answer was suggested to him "in part by a sentence which the late Preserved Smith wrote in 1930,—'suddenly within two years, appeared three of the most momentous works of science that the world has ever seen, Copernicus *On the Revolution of the Heavenly Orbs* (1543), Vesalius *On the Structure of the Human Body* (1543), and Cardan's *The Great Art* (a treatise on algebra, 1545).'" While two of these scientists have grown with the centuries in the esteem of men, Cardan, though not forgotten, has become a dimmer figure as the years go by. Dr. Eckman feels that "to appraise such a man who assuredly contributed much to the advancement of learning, and who nevertheless in modern times seems to be almost bereft of the fame he so greatly desired, is an undertaking which appears not to be wholly without profit."

Neither Cardan's *Vita propria* nor any of the longer biographies gives so clear a picture of his career as the biographical sketch *Pauca de vita eius* which constitutes the second chapter of this monograph. To the chronology and to the sequence of events and activities of Cardan's life the author has given scrupulous examination. Abundant references and footnotes give evidence that he has overlooked few sources of information which might aid in ascertaining the

facts. Around this record we see the environment within which it was realized and which gives meaning to all Cardan did or wrote.

The impact of the struggle which Charles V and Francis I waged for the prize of North Italy fell heavily upon Cardan's early manhood, but not always unfortunately, for when war closed the University of Pavia he went to Padua, "which enjoyed an international reputation unequalled by any other university of its time." From slow and meager beginnings the young physician, fighting against the poverty and prejudices of his social setting for a living, for recognition, and for acceptance, rose to final renown. If the man Cardan is not as distinct as the record of his doing, the difference may be laid to the limitations Dr. Eckman has set upon his theme. The picture of Cardan's seventy-five years, touched by the sorrows and satisfactions of living, and rich in achievement, would doubtless satisfy Cardan, whose ambition, according to Sir Thomas Browne, was "to be content that times to come should only know there was such a man, not caring whether they knew more of him."

The splendor of the Renaissance, the discoveries of the New World and its fabled treasures, the conception of nations, the Reformation, the deeds and accomplishments of some of the world's most remarkable men, and the force of striking events that were to mark deeply the course of human history all flowed magnificently together into the high tide of the times in which Cardan lived, *Cardani aevum*. Dr. Eckman observes the working of these grand influences on Cardan. More particularly he studies him in relation to his associations in the two great universities, Pavia and Padua, where Cardan both studied and lectured, and in his relations with distinguished men all over Western Europe.

This chapter on the Age of Cardan the reviewer finds the most provocative of the monograph; here are seen not the celebrities of the century, but the more obscure figures of scholars and thinkers, select men everywhere, stirring and doing, and, what is more exciting, finding each other across dividing barriers. One would like to hear more of the process! To men like these Cardan was known in his own lifetime and far beyond his native Italy, and through them he "must have exerted considerable influence on the scientific and philosophic thought of the sixteenth century."

Judicium de eo, an evaluation of Cardan and his achievements, concludes Dr. Eckman's study. In this he declares that he "has no objective other than to arrive at an approximation of the truth." To his knowledge "no unfavorable testimony has been suppressed," and he "hopes that at the other extreme the evidence adduced has not been too sanguine."

This *judicium de Cardano* promptly addresses itself to the inevitable triangle of the Cardan-cube of the equation-Tartaglia controversy. The author finds that Cardan's contemporaries and immediate successors were not im-

pressed with the quarrel; the legions of those who attacked Cardan struck at a later period and with a prejudice blind even to Cardan's acknowledgment of his debt to Tartaglia. Recent criticism, of which Bortolotti,[1] writing in 1926, is an instance, rather vindicates Cardan in his procedure and takes a more lenient attitude toward his undeniably flagrant breach of faith. But Cardan's eminence as a mathematician does not rest only on this classic "solution" of the cube of the equation. The late Preserved Smith's high estimate of the *Ars magna* has been quoted earlier in this review.

In medicine Cardan has won just esteem. He has been named a pioneer in the field of psychiatry and has been "associated with the belief that the seeds of disease are actually living." In pathologic anatomy and teratology he did important work, and he appears to have been the first to describe typhus fever and to differentiate it from measles. To him are attributed the origin of a method for teaching the deaf and the recognition of the significance of epilepsy in melancholia. The sum of his contributions, to which the author gives weight by copious references, is imposing; what is more, through suggestion Cardan became the forerunner of several valuable discoveries and processes. The ideas of his busy brain were widely sown to become fertile where they would. Even an invention which he never claimed bears his name today, Cardan's shaft, the indispensable universal joint.

Cardan was intolerant of classic authority in every area of scientific inquiry. In the light of this protest against antiquity Dr. Eckman examines the claim of many of his admirers that Cardan deserves a place among the modern formers of philosophy. No more than he felt fettered by the past did he feel circumscribed by geographical limitations. His mind took flight to lands beyond the borders of Italy or even Europe; Brazil, Peru, Mexico, and Florida were to him known and nameable regions.

Dr. Eckman rises to an almost partisan defense when he deals with the "irrational objections to Cardan" based on his devotion to astrology, his superstitions, his belief in familiar spirits and magic, his impiety or atheism, and his questionable sanity. He urgently wishes us "to view the man himself against the background of his time" and to realize that Cardan was far from solitary in these credulities and failings. He arrays a whole company of very illustrious men who were subject to some of the same superstitions and weaknesses, among them a president of Harvard College. He points out that an *Introduction to Astrology* by Lilly, who published Cardan's *Aphorisms* in his *Astrologer's Guide,* was issued in the United States as late as 1935.

[1] Ettore Bortolotti, *Studi e memorie per la storia dell' Università di Bologna* 9 (1926) 57-108.

The charge of atheism or at least impiety, springing partly from Cardan's writings and partly from the horoscope of Christ which he cast, is reviewed and discussed in careful detail, as well as the question of Cardan's sanity. All evidence weighed, it appears that Cardan remained a loyal, if not a fervent Catholic to the end of his days. The author concludes that "the difficulties of judging the mental competence of a man dead 370 years, on the basis of the man's written works and the testimony of those who lived in an age steeped in superstition and credulity, seemed insurmountable."

Never has this maligned and often misjudged man had a fairer trial than Dr. Eckman has essayed. To it he has brought, as Dr. A. C. Krey of the University of Minnesota has pointed out in the Foreword, the knowledge of science, of languages, and of history, an essential pre-requisite for those who would do effective work on the history of medicine. The generous use of footnotes shows how indefatigable Dr. Eckman has been in the pursuit of truth and how unwearied in research; throughout this whole thorough and scholarly appraisal he has given the reader the benefit of a commentary entirely appropriate to a work of this character. These notes, as well as 335 references appended to the body of the monograph, constitute the author's "chapter and verse" for practically every statement and claim in his compendious modern study of Jerome Cardan.

JEAN STONER

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The Centennial of Surgical Anesthesia. An Annotated Catalogue of Books and Pamphlets Bearing on the Early History of Surgical Anesthesia. Compiled by JOHN F. FULTON, M.D. and MADELINE E. STANTON, A.B. New York: Henry Schuman, 1946. 102 pp. $4.00.

EXHIBITED at the Yale Medical Library in October 1946, this reprint appears as Publication Number 15 in the series of the Historical Library.

The compilation reviews the development of the concept of man's struggle against pain. As pointed out in the introduction in surveying the early literature of surgical anesthesia it is essential to maintain complete objectivity; the selection appears to be excellent in this catalogue. A review is included of the forerunners of surgical anesthesia, sleeping potions, chilling with snow and ice, mesmerism, and hypnotism. The original papers of Crawford Long and Horace Wells and the Morton-Jackson controversy are given in Chapters II to V. Simpson's experience with chloroform is adequately covered in Chapter VI. A complete bibliography is included with each chapter. Regional and block anesthesia

and anesthetics discovered after 1875 will be dealt with in Part II of this catalogue.

Like its companion volume, *Morton's Memoir on Sulphuric Ether*, this compilation will find a popular place in the libraries of those interested in the History of Medicine.

C. Jelleff Carr

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A Memoir to the Academy of Sciences at Paris on a New Use of Sulphuric Ether. By W. T. G. Morton, Boston, U.S.A. 1847. With a Foreword by John F. Fulton, M.D. New York: Henry Schuman, 1946. 24 pp. $1.50.

This reprint appears as Publication Number 14 in the Historical Library Collection of the Yale Medical Library. Dr. Fulton has added a short descriptive foreword.

To those interested in the historical background of great medical discoveries this little publication will be welcome. To students of this subject the reprinting of the celebrated letter of Morton to the Academy of Sciences at Paris provides an appropriate testimonial on the occasion of the ether centennial year. Most physicians will wish to have a copy of this *Memoir* in their library.

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George Urdang. *Pharmacy's Part in Society.* With a Foreword by Robert P. Fischelis. Madison, Wisconsin: American Institute of the History of Pharmacy, 1946. 93 pp. $5.00.

In this interesting volume the writer purports to take one through the corridors of the past and emphasize the important role that has been played by the pharmacist in the various ramifications of the social structure. The evolution of the pharmacist from the alchemist, the separation of medicine and pharmacy, are given in beautiful illustrative cuts and also told with interesting style.

The writer shows the part that pharmacy played in the beginnings of scientific periodicals. The important contributions that pharmacists have made in public health, their contributions to botany and plant chemistry are given in great detail. In more modern times, the writer shows the important efforts of the pharmacist in plant chemistry, and devotes much time to the work of

Pelletier and Caventou and the other pharmacists who in the early part of the nineteenth century were successful in isolating the alkaloids from various plants.

A very interesting discussion in this book deals with the men who have distinguished themselves in other lines of work, but who started out as pharmacists. Among these we note O. Henry the writer, David Henshaw the Boston statesman, and the Dutch pharmacist, J. P. Minkelers, who obtained illuminating gas from coal. The last section of the book is devoted to the role of the pharmacist in general culture, and much attention is given to the literary works of John Uri Lloyd, enclosing a cut of Lloyd's most popular book, Felix Moses the Beloved Jew of Stringtown on the Pike.

In all, the book is interestingly written and will serve as a great inspiration to young men who are considering pharmacy as their career and certainly will be a source of gratification to any pharmacist to know of his illustrious heritage.

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CALVIN F. SCHMID. *Social Trends in Seattle.* Seattle, Washington: University of Washington Press, 1944. University of Washington Publications in the Social Sciences, Volume 14. xi + 336 pp. $3.75.

THE history of medicine may be approached from various points of view—anecdotal, biographical, the history of techniques, or ideas—each useful within its limitations. Each of these approaches may be considered as a *level of integration* within a *comprehensive* medical history. At the level of such comprehensive history, the approach of the historian must be a social one, that is, he must regard the various facets of medical history within the framework provided by society as a whole. For the historian to become acquainted and accustomed to this type of thinking it may be useful for him to go outside the narrower boundaries of medical history and to study a volume such as Dr. Schmid's *Social Trends in Seattle.*

In this monograph, Dr. Calvin F. Schmid, Professor of Sociology in the University of Washington, has thoroughly analyzed and summarized the complex condition and past development of the city of Seattle. In separate chapters he considers the factors in the growth of Seattle; the process of city expansion; trends and characteristics of the population according to sex, age, nativity, educational and occupational status; mortality trends; housing conditions; and political trends and characteristics. The city of Seattle is seen as a whole, thus providing a framework into which the medical history of this

community could be fitted. The same thing can of course be done on a more comprehensive scale.

Dr. Schmid's monograph is recommended to medical historians as an example of method, but I am certain that they will also find the contents of great interest.

GEORGE ROSEN

RICHARD UPJOHN LIGHT. *The Progress of Medical Geography;* J. K. WRIGHT. *A Proposed Atlas of Diseases with a Note on the Terminology of Certain Map Symbols.* Reprinted from *The Geographical Review* 34 (1944) 636-654, American Geographical Society, Broadway at 156th Street, New York 32, N. Y.

INTEREST in the relation of geographic factors to health and disease goes back to antiquity, traditionally to the Hippocratic treatise *On Airs, Waters and Places.* Nevertheless, it was not until the first half of the eighteenth century that attempts were made to collect the fragmentary available material on the occurrence of diseases in various parts of the world. Friedrich Hoffmann (1660-1742) and Johann Friedrich Cartheuser (1704-1777) made the first, but relatively unsuccessful attempts to utilize such material for a synthesis of medical geography. This goal was first achieved by Leonhard Ludwig Finke (1747-1828 or 1829) with the publication of his *Versuch einer allgemeinen medicinisch-praktischen Geographie. . . .* (3 vols. 1792-1795). Finke was followed by numerous other writers and students in the field of medical geography.

Throughout most of the nineteenth century, there was an active interest which manifested itself in the publication of numerous medical geographies, and an even larger number of studies more limited in scope, known as medical topographies. Among these were Friedrich Schnurrer (1784-1833) who in 1813 published a *Geographische Nosologie;* J. C. M. Boudin who in 1843 published his programmatic *Essai de geographie médicale,* and in 1857 his great medical geography in two volumes, *Traité de geographie et statistique médicales et maladies endémiques;* C. F. Fuchs with his *Medicinische Geographie* (1853); A. Mühry with his *Geographische Verhältnisse der Krankheiten . . .* (1856); and the famous August Hirsch with his *Handbuch der historisch-geographischen Pathologie,* first published in 1860, and reissued in a second edition of three volumes between 1881 and 1886.

With the arrival of bacteriology, most workers felt that environmental factors might be discounted in the causation of disease, and consequently lost interest in medical geography. One field of geographical medicine was still kept alive. With the development of colonial empires, health conditions in these areas became matters of importance and were subjected to study, leading to the development of "tropical" medicine.

Interest in medical geography revived, however, during the second and third decades of the present century. Askanazy succeeded in organizing the International Society of Geographical Pathology in 1929, and in convening the first international conference on this subject at Geneva in 1931. At the same time Henry E. Sigerist and Fielding H. Garrison urged the importance of medical geography.

Until the last decade, the United States has not been particularly interested in the field of medical geography. (During the nineteenth century, numerous topographies were written—the greatest of them being Daniel Drake's classic *On the Principal Diseases of the Interior Valley of North America,* 1850.) In the early 'thirties, a survey was undertaken under the auspices of the National Research Council (Division of Medical Sciences) and a volume entitled *A Geography of Disease* was issued, but the study was very disappointing. Recently (1944), the first volume of a collaborative work on *Global Epidemiology* was published, and promises to be of some significance in the development of medical geography.

It is against this background that one must consider the record of the conference held by the American Geographical Society on May 20, 1944, of which a report is given in the brochure under review. The American Geographical Society had long felt the desirability of initiating and publishing studies in the field of medical geography. At the meeting of the Council of the Society on February 29, 1944, Dr. Richard U. Light suggested the possibility of preparing an atlas of diseases. The Council then authorized the holding of a conference of medical men and others to advise the Society on the feasibility of such an undertaking. This Conference was held on May 20, 1944. Short introductory talks were made by Dr. Light and Dr. J. K. Wright, the latter explaining some technical problems of map construction with particular reference to scale, projection, symbols, etc. The conference reached the following main conclusions: That the atlas would be designed primarily as a tool of research, although certain parts might be adapted to the practical needs of individuals and institutions coping with the control of diseases under special conditions; that it would be issued in separate parts that could be revised as new data became available; and that ten diseases be included in the atlas—cholera, the typhus group, malaria, yellow fever, goiter, plague, beriberi and pellagra, filariasis, encephalitis, and schistosomiasis. It is interesting to note that the inclusion of historical maps was agreed upon as long "as they are of real aid to research." This atlas, when it appears, will undoubtedly be a very useful tool. One hopes that in its preparation, medical historians who have devoted attention to the history of disease and of medical geography, will be consulted.

GEORGE ROSEN

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